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ORIGINAL ARTICLES.

I.—Woodwardian Museum Notes.

By F. R. Cowper Reed, B.A., F.G.S.

(PLATE VII.)

THE sub-genus *Chasmops* of the genus *Phacops* is so rarely, if ever, found in great Britain, except as disconnected heads and pygidia, that a specimen which shows the cephalic shield attached to several thoracic rings deserves notice.

The two British species recognised by Salter (omitting the doubtful *Ch. Jukesii*) are *Ch. macroura* (Sjogren) and *Ch. conophthalmus* (Boeck ?),[1] but he apparently perceived that more than two species were included under these names (Mon. Brit. Tril. p. 38). The material in Britain available at that time, however, was not well enough preserved, and the investigation of the Continental forms of this characteristically North-European group was not sufficiently advanced to admit of a more complete discrimination. But now Fr. Schmidt's work on the Silurian trilobites of the East Baltic provinces (Mém. l'Acad. Imp. d. Sc. d. St. Petersb. vii. ser. T. xxx. No. 1) has supplied a long-felt want, and we can only regret that the British specimens of *Chasmops* generally occur in such an unsatisfactory and fragmentary condition as to preclude their indentification or comparison with those he has so fully described. The material with which he has had to deal is in a comparatively excellent state of preservation, and it is gratifying to find an English specimen which does not unfavourably compare with some of his. Such is the case with the one now described. It was discovered by Mr. F. Marr in the Coniston Limestone of Applethwaite Common near Windermere.

The head and 7–8 thoracic rings are preserved, but they have suffered considerable lateral compression, producing irregular cracks and corrugations of the surface together with some distortion. For instance, the parabolic shape of the cephalic shield is misleading, for probably it was originally semicircular and the proportion of the length to the breadth about 1 : 2. In the thorax the axis has been squeezed into greater prominence, and on the pleuræ antero-posterior wrinkles have been developed. The actual measurements of the specimen in its present state are as follows:—

[1] According to Fr. Schmidt, Salter's species are not those of Sjogren and Boeck.

Length of head	1·35	inch
Length of glabella from neck-furrow to anterior end	1·1	,,
Width of broadest part of glabella, *i.e.* across frontal lobe	1·1	,,
Width of base of glabella	0·35	,,
Length of the 1st or "cat's ear" lobe, measured along its outer side	0·575	,,
Width of right cheek measured along the neck-furrow from the axial furrow to the outer margin	0·9	,,
Length of eye	0·375	,,
Width of eye	0·175	,,
Length of channel round eye	0·5	,,
Width of ditto	0·225	,,
Width (average) of thoracic axis	0·5	,,
Length (average) of pleura	0·8	,,
Width (average) of ditto	0·2	,,

The accompanying description refers to the crushed and distorted specimen figured. (A summary of the chief characters of the form with the proportions, which it must have possessed prior to the distortion, is given subsequently.)

The outline of the head is parabolic; the anterior end appears to be obtusely pointed in front of the glabella. The glabella is much dilated in front, and the sides converge at an angle of about 40°, but before distortion this angle must have been about 60°, and the outline of the head must have been semicircular.

The frontal and first lobes of the glabella are well marked off by deep furrows. The frontal or forehead lobe is transverse, the width $1\frac{1}{2}$ times the length, but originally it must have been about twice as wide as long; the anterior border is a gentle convex-forward curve; the lateral angles of this lobe are rounded-truncate, and overhang the eyes completely, projecting beyond the straight outer side of the "cat's ear" lobes.

A Vishnu-like mark formed by two converging rows of large puncta, which may represent the position of frontal glands, extends two-thirds down the lobe from the anterior border.

The triangular "cat's ear" or first side lobes extend fully two-thirds of the side of the glabella, reaching nearly from the neck-furrow to a point in front of the eye; each of these lobes stretches nearly one-third of the way across the glabella. The largest angle of each is about 110° and points inwards, but before distortion must have approached a right angle. The anterior bounding furrow is rather sinuous, but the posterior one is straight.

In the triangular portion of the glabella left between these large embracing lobes, the second or middle lobes are represented by small, very faintly indicated, subcircular elevations, more clearly defined in front than behind, and only perceptible in the impression. The basal or third lobes are larger and transverse, but are poorly defined, and together with the middle ones are completely embraced laterally by the large first lobes. (This feature also is shown better in the impression than in the cast.)

The glabella as a whole is not particularly inflated, and, in fact, is not so high as the inner portions of the cheeks; but this is probably due in the main to the crushing to which it has been subjected.

The neck-segment is only about 0·15 inch broad, and is strongly rounded and prominent; it is separated by a strong neck-furrow from the base of the glabella, and this furrow is continued laterally as a deep narrow groove limiting the cheeks posteriorly, but it shallows and terminates before reaching the genal angles.

The cheeks are triangular, not more convex than the glabella but higher along the posterior portion of their inner border. They terminate at the genal angles in long flat spines, closely pressed against the body, and grooved by a continuation of the marginal furrow. At their fixed end they are of considerable width, but narrow at first rather rapidly to about the level of the second body-ring, beyond which the tapering is very gradual. The left spine is partly preserved and reaches down the whole seven and a half thoracic rings on that side, at which point it is broken off.

The axial furrows are narrow, but deep and straight as far forward as the anterior end of the first lobe. The cheeks rise steeply from them. At the anterior end of this first or "cat's ear" lobe the furrow curves outwards and widens into a broad groove, bounding the cheek anteriorly and overhung by the great frontal lobe of the glabella.

From the highest portion of the cheek which is adjacent to the axial furrow there is a gentle slope down to the outer margin, where a narrow rounded groove—the marginal furrow—bounds it. Outside this furrow is a broad, slightly convex, smooth border, 0·2 inch wide; this is prolonged backwards into the genal spines, and anteriorly runs round the front of the head-shield, where it is reduced to a quarter of its width by the overhanging of the frontal lobe of the glabella.

The eyes are large and lunate, extending nearly $\frac{2}{3}$ the length of the triangular first glabellar lobes. A deep crescentic channel bounds them externally, sweeping round their base. The posterior end of each crescent is about half the length of the eye from the axial furrow, and the anterior end is almost on this furrow.

The cheeks and glabella are finely granulated and covered with small round tubercles, the largest of which are situated on the triangular lobes and the portion between them. The smooth, non-tubercular character of the border offers a striking contrast.

The facial suture is seen to curve round the front of the glabella and, apparently, is supra-marginal, but this part of the specimen is not well preserved. From the rounded lateral angles of the great frontal lobe this suture runs backwards and inwards in a nearly straight line to the eye, at the posterior end of which it bends outwards in a convex-forward curve and appears to lie in a slight groove. It cuts the outer margin at a point almost on a level with the posterior end of the eye.

The axis of the thorax is very convex and is of nearly uniform width, which is rather more than half that of the pleuræ. The axial furrows are deep. The pleuræ are flattened; the fulcrum is well marked and is situated at about one-third of their length from the axial furrow. Each pleura is horizontal and at right angles to the

axis as far as the fulcrum, but at this point it bends backwards at an angle of from 30° to 40° to its previous direction, and curves gently downwards to its free end, which is blunt, rounded, somewhat flattened and expanded. A deep furrow traverses each pleura obliquely from the inner anterior angle towards the middle of the free end, before reaching which, however, it dies out.

Where the crust has been preserved a few tubercles and granules are seen to occur on the axis and pleuræ, but they are not so large or so numerous as on the head.

Summary of the normal characters of this form (undistorted).

Head-shield semicircular, with an obtuse point in front. Posterior margin forms a concave-backward curve to the large spines at the angles.

Glabella much dilated in front, moderately inflated; base measures about a quarter the width of head-shield, but only one-third the greatest breadth of glabella which is across transverse frontal lobe. The sides converge posteriorly at an angle of about 60°. Frontal lobe transverse, twice as wide as long; anterior border a gentle convex-forward curve; lateral angles rounded-truncate overhanging the eyes; a V-shaped series of puncta present on anterior portion of this lobe. First side lobes ("cat's ear" lobes) extend from neck-furrow to a point in front of eyes, *i.e.* for two-thirds the side of the glabella; are triangular; inner angle about 90°; reach nearly one-third across the glabella; anterior bounding furrow sinuous; posterior furrow straight. Second or middle lobes very faint, subcircular; third or basal lobes rather more definite and wider. Both second and third pairs of lobes completely embraced laterally by first lobes. Axial furrows of glabella deep, narrow, straight as far as anterior end of first lobe, then widen and curve outwards to join marginal furrow.

Neck-segment rounded; neck-furrow deep, dies out before reaching marginal furrow.

Cheeks triangular, slightly inflated, highest along inner border.

Genal angles terminated by large flat spines, broad at base, slightly grooved, closely pressed against body-rings, extending to 8th or 9th thoracic segment.

Marginal furrow narrow, smooth, and rounded, with smooth, wide, slightly convex border external to it and forming edge of head-shield. Anterior portion of border overhung to the extent of three-fourths of its width by frontal lobe of glabella.

Eyes large lunate, near inner margin of cheeks; extend nearly two-thirds the length of the first side lobe of the glabella; outer side of base of eyes surrounded by deep crescentic channel.

Cheeks and glabella finely granulated and covered with small round tubercles, especially conspicuous on posterior part of glabella.

Facial suture supra-marginal; below eye it makes a convex-forward curve and ends on outer margin at about the level of the posterior end of the eye.

Thoracic axis about one-fifth the width of thorax; of uniform width; axial furrows deep.

Pleuræ flattened, with deep oblique furrows; fulcrum conspicuous, situated at $\frac{1}{3}$ length of pleura from axial furrow.

Pleura horizontal and at right angles to axis as far as fulcrum; then bend back at 30°—40° to previous direction and curve slightly downwards.

Extremities of pleuræ blunt, rounded, flattened.

Crust of thorax slightly granular with few tubercles.

The form above described does not exactly agree with Salter's *Ch. macroura* (Mon. Brit. Tril. p. 37), but resembles it in the general shape and tubercular character of the head, in the cheek spines, and the course of the facial suture; but the lobes of the head, the size of the eye, and the cephalic border show important differences.

With *Ch. conophthalmus*, as described by Salter (*loc. cit.* p. 40), it has the transversely overhanging frontal lobe in common; but there are considerable points of dissimilarity in the length of the genal spines, the tuberculate condition of the head, the size of the eyes, the presence of the middle and basal lobes of the glabella, and the non-continuity of the neck-furrow with the marginal furrow.

The absence of any description by Salter of the thoracic rings of these species, and the fact that the tail of our specimen is wanting, prevent us making a more complete comparison; but so far as the comparison goes we see that our form cannot be said to agree with either of Salter's species. On turning to Schmidt's Memoir on the Silurian Trilobites of the East Baltic provinces we find there are three species with which it has several features in common. In the first place a comparison may be made with *Phacops* (s.g. *Chasmops*) *Eichwaldi* (Schmidt) (*loc. cit.* p. 117). The figures of this form given by Schmidt (taf. v. f. 8 *a. b. c.* 9) do not bear much resemblance to our Coniston Limestone specimen, but in the description of the species the characters, shape, and size of the frontal lobe of the glabella are similar, as are also the shape of the first side lobe (in *size* it seems rather shorter), the cheek margin, and the length, shape, and position of the genal spines. But there are considerable differences in the parabolic shape of the head-shield (which in our specimen is only due to crushing as has been pointed out), the finely granular, non-tuberculate or smooth surface, the total absence of the second side lobe, the more marked character of the third side lobe, the smallness of the eyes, the swelling out and subsequent tapering of the thoracic axis, and the scarcely perceptible fulcrum of the pleuræ.

Schmidt thinks that his *Ph. Eichwaldi* is very similar to or even identical with Salter's *Ph.* (*Ch.*) *macroura*.

Another species—*Ph. maxima* (Schmidt) (*loc. cit.* p. 112)—agrees in the shape of the head-shield, the character of the genal spines, and the size, shape, and extent of the first side lobe; but it differs by having only fine granulations and no tubercles, by the marginal furrow uniting with the neck-furrow, by the second and third side

lobes being much more conspicuous, and by the greater breadth of the frontal lobe.

The third species deserving comparison is *Ph. Wesenbergensis* (Schmidt) (*loc. cit.* p. 115). This bears the closest resemblance to our form. The shape of the head, the genal spines, the granular and tubercular surface of the glabella, the border to the head-shield, and the second and third side lobes of the glabella are all very similar. The first lobe is usually smaller, but Schmidt states that its form varies considerably. The most important points of difference are the smaller size of the eyes, the non-tuberculate character of the cheeks, and the absence of a conspicuous fulcrum to the pleuræ.

On the whole it seems, therefore, justifiable to consider our Coniston Limestone form specifically distinct from any hitherto described; and in honour of its discoverer it may suitably be named *Ph. Marri*.

EXPLANATION OF PLATE VII.

Fig. 1. Natural cast of *Phacops* (*Chasmops*) *Marri* from the Coniston Limestone, Applethwaite Common, Windermere. Natural size.
Fig. 2. Impression of ditto.
Fig. 3. Side view of figure 1, showing genal spine.
Fig. 4. *Dindymene Hughesiæ* (Roberts MS.), Coniston Limestone Series, Norber Brow.
Fig. 5. Impression of ditto.

N.B.—By mistake the figure of the type specimen on which the restoration of *Dindymene Hughesiæ* was founded (Geol. Mag. Dec. IV. Vol. I. No. III. Pl. IV. Fig. 5) was omitted from the plate which accompanied the description in the March Number. It is, therefore, here inserted.

II.—Note on Some Appendages of the Trilobites.[1]

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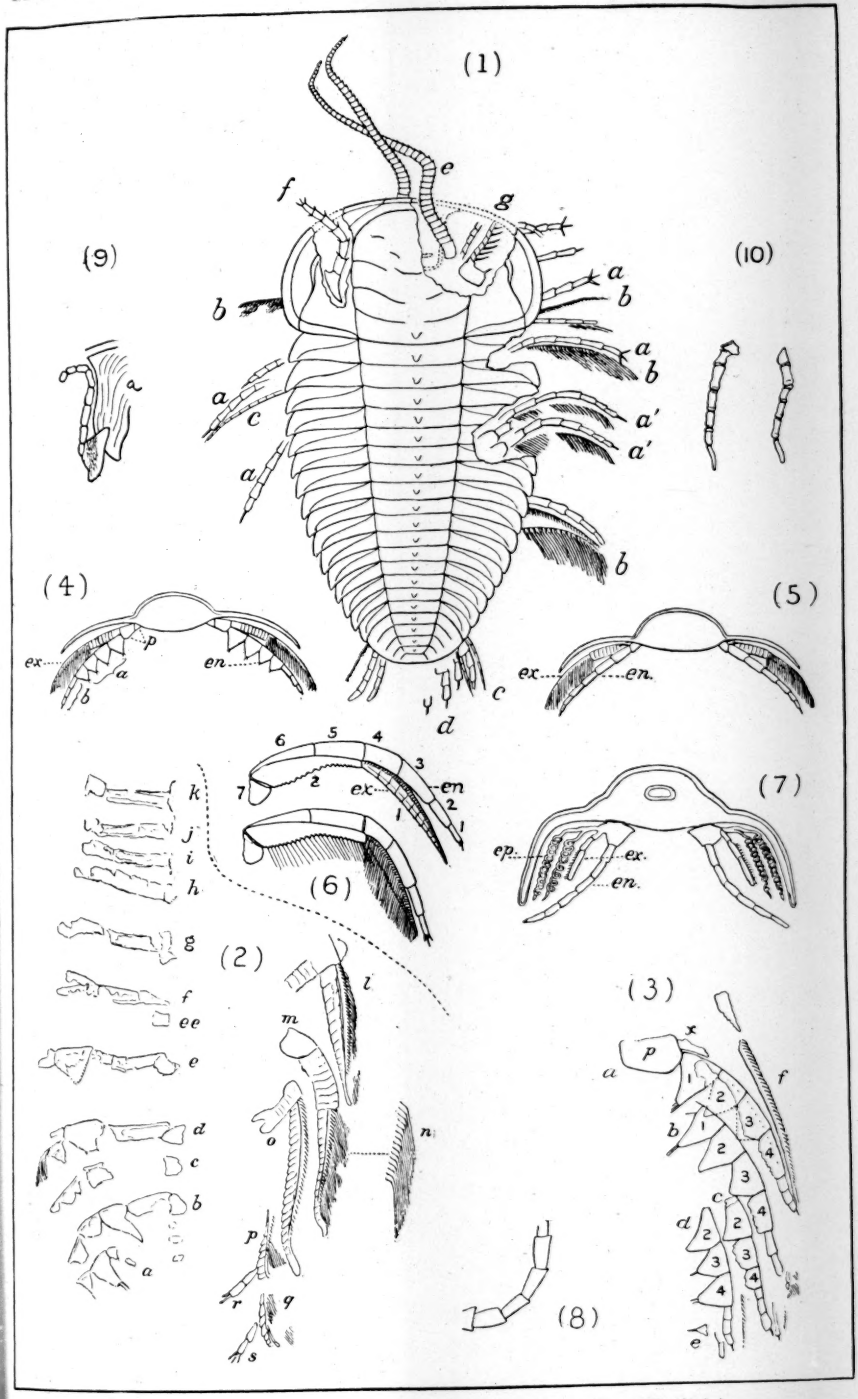
(PLATE VIII.)

THE results of Mr. W. S. Valiant's long search for the appendages of Trilobites have recently been made known by Mr. W. B. Matthew, who described the material sold to the Columbia College of New York by Mr. Valiant.[2] Mr. Valiant informs me that he discovered traces of what he considered to be antennæ, and that for several years he continued collecting until he found a locality where the specimens were well preserved and showed, not only the antennæ, but legs and what he supposed to be the swimming appendages. Not having confidence that he could properly describe the specimens he sold part of his material, and in this way it came to be first described by Mr. Matthew, a student at Columbia College. His step-brother, Mr. Mitchell, continued to collect; and in August, 1893, through the courtesy of Mr. Valiant, I visited the locality

[1] Read before the Biological Society of Washington, March 24, 1894.
[2] Amer. Journ. Sci. 1893, vol. xlvi. p. 121.



Appendages of *Triarthrus Becki*, and other Trilobites.

with Mr. Mitchell and obtained a few specimens for the National Government.

The most important part of the discovery, announced by Mr. Matthew's paper, is that the Trilobita have true antennæ. The discovery of the legs and plumose appendages is also of great interest, as it adds to our information respecting the appendages of the Trilobite some of the details of another genus.

A collection was made for the Yale College Museum by Dr. C. E. Beecher, and in some notes on the thoracic legs of *Triarthrus*[1] he describes and illustrates a dorsal view of the legs of the second and third free thoracic segments. These show that the endopodite of the leg is essentially the same as in *Calymene* and *Asaphus*, and that the exopodite is unlike that of *Calymene* or *Ceraurus*.

Through the courtesy of Prof. J. F. Kemp, of Columbia College, I have examined the material studied by Mr. Matthew; and Prof. A. H. Chester, of Rutgers College, kindly loaned me for study five specimens that he purchased from Mr. Valiant. From these and the specimens in the National Museum a few notes have been taken that permit of some comparisons with the extremities found in *Ceraurus*, *Calymene* and *Asaphus*.[2] The limbs of *Triarthrus* differ in the details of the joints of the inner branch of the limb (endopodite) and still more in the character of the exopodite.

Cephalic limbs.—The antennæ are uniramose, and, judging from the position in which they are found, were attached to the body near the postero-lateral angle of the hypostoma (Fig. 1, *e*, Pl. VIII.). In one specimen a cephalic limb somewhat detached from its true position shows a large basal joint and six slender joints (Fig. 1, *f*). The basal joint does not show conclusive evidence of the presence of a masticatory ridge. On another specimen, however, the form of the basal joint strongly suggests that it subserves the purpose of mastication. This is illustrated at *g* in Fig. 1.

A slender jointed appendage like that attached to the basal joint of *g*, occurs between it and the antenna, and is probably a portion of another one of the cephalic limbs. No other cephalic appendages have been observed in the material at hand.

Since the publication of my articles on the Trilobite[3] I found in a section of the head of *Calymene senaria* a slender jointed limb that appears to have been an antennule. It is unlike any limb found beneath the head and thorax, and, if not an antennule, it may represent a fifth pair of cephalic limbs. This is also suggested by a section of the limbs within the head of *Calymene* (illustrated on pl. 1, fig. 9, Bull. Mus. Comp. Zool. 1881, vol. viii.). In this, a fifth limb is indicated close to the hypostoma. The Trilobite was enrolled so as to include the antennule entirely within the border of the head. A sketch, taken from a photograph of the thin section by transmitted light, is shown by Fig. 8, Pl. VIII.

[1] Amer. Journ. Sci. 1893, vol. xlvi. pp. 467–470.

[2] The Trilobite; New and Old Evidence Relating to its Organization. Bull. Mus. Comp. Zool. 1881, vol. viii. p. 6.

[3] Bull. Mus. Comp. Zool. 1881, vol. viii. p. 191–224. Science, 1883, vol. iii. p. 279.

The hypostoma of *Ceraurus*[1] shows a rounded indentation of the antero-lateral sides, where an antennule probably passed by it. This character is strongly marked in *Sao hirsuta, Proetus bohemicus, Amphion fischeri*, etc., as illustrated by Barrande.

The character and position of the remaining cephalic limbs of *Triarthrus* are not shown in any specimens that I have examined; but, from the relations of *Calymene, Ceraurus* and *Triarthrus*, especially the two latter, it is probable that their arrangement is essentially the same.

Thoracic limbs.—Many specimens show the thoracic limbs extending out from beneath the carapace of *Triarthrus*. It was not until by a fortunate dissection that I obtained the material illustrating the limbs in position beneath the thorax. The anterior limbs are formed of a protopodite and a somewhat complex exopodite. The protopodite consists of a short basal and a long joint (Fig. 2, *d, e*), to which the endopodite and exopodite are attached. This appears to be direct in the posterior limbs of the thorax (Fig. 3, *a*), but as yet the point of attachment of the basal joint of the exopodite has not been seen in the anterior limbs.

The endopodites of the anterior thoracic limbs vary in the number of joints and in their relative length (Fig. 1, *a, a*). Two show four long proximal and three shorter distal joints. Other limbs show two smaller distal, and three or four proximal, while in several there is a more or less uniform gradation from the protopodite to the distal joint. In Fig. 1, some of these variations are indicated. In Fig. 2, eleven limbs are shown, as seen from the under side. The basal (coxal) joint is seen at *b, d, e*, and nine show the long second joint of the protopodite. At *e* and *f* a new phase is indicated by the enlargement of the proximal joints. This is marked in *a, b, c, d*, and in Fig. 3 the details are more fully shown. These joints occur on the seven posterior thoracic limbs of Fig. 2; and in the specimen from which Fig. 3 was drawn the limb opposite the the tenth segment from the pygidium shows a slightly triangular *second* (meropodite) and *third* (carpopodite) joint. In Fig. 2, the limb *a* is opposite the second free segment of the thorax anterior to the pygidium. The limbs *a* and *b*, Fig. 3, clearly show that the four proximal joints are broad and subtriangular in outline. A glance at the abdominal swimming legs of the Phyllocarida (*Paranebalia*), Schizopoda and Cumacea, suggests that the functions of these legs were both natatory and ambulatory.

The expododite illustrated by Beecher shows the dorsal surface (Fig. 6). A number, presenting the ventral surface, are shown on the right side of Fig. 2. They occur on the same specimen as the endopodites, on the left side, but have been pushed out of place. The most perfect is represented by *m*. The proximal portion is formed of a rather large basal joint and a number of short joints, seven or eight. The distal end is formed of an inner and outer segmented portion. The inner side is divided into numerous segments by oblique divisions that give the impression of a closely

[1] *Loc. cit.* pl. iv. fig. 5.

coiled spiral. The outer side is a cylindrical, jointed, stem-like rim that is attached to the inner side, a narrow distinctly impressed line separating the two, except at the somewhat flattened tip where they merge into each other. On the outer or upper surface of the outer side numerous crenulations occur that extend into long setæ, *n*, Fig. 2; *b*, *b*, Fig. 1. Dr. Beecher considers the expodite as a swimming organ; but from the manifest branchial character of the exopodite and attached epipodite in *Calymene* (Fig. 7), it seems probable that this exopodite of *Triarthrus* served largely as a gill, and that the animal used the broad proximal joints of the posterior limbs of the thorax as its principal propulsion in swimming. The exopodite of *Triarthrus* looks like a consolidated exopodite and epipodite, very much as though these two organs, as they occur in *Calymene*, were merged into one.

Several specimens illustrate appendages beneath the pygidium. Some have the broad proximal joints, *d*, Fig. 1, while others show the outer rim of the exopodite, *c*, Fig. 1. The material I have seen indicates very little difference between the appendages of the posterior half of the thorax and the pygidium, except that those of the latter are less developed in size and details.

Mr. Matthew suspected the presence of a flap, formed by the anchylosing of the appendages beneath the pygidium. From the appearance of a similar structure, where the limbs are matted together along the side of the thorax, this tentative view is received with doubt. More perfect material may show distinctions not recognizable at present.

If future investigations prove, as it now seems probable, that the modified swimming joints of the endopodite are attached to ten or more of the thoracic segments, the anterior eight segments can be grouped together as the typical thorax, and the remaining segments of the body as the abdomen.

Mr. Matthew suggests that the homology between *Triarthrus* and *Limulus* may not be as close as between *Limulus*, *Calymene* and *Ceraurus*. This is true from what we now know of *Triarthrus*, but, if a sixth pair of cephalic limbs should be discovered in *Triarthrus* the resemblance would be strengthened. *Triarthrus* does not differ from *Ceraurus* and *Calymene* more than would be anticipated in such unlike genera. *Triarthrus* is essentially a "Primordial" type that has continued until Upper Ordovician time. It represents a large group of Cambrian Trilobites, whilst *Calymene* and *Asaphus* represent the more highly developed Ordovician and Silurian forms.

Dr. Lang held the view[1] that if a fifth pair of cephalic limbs were found, comparable to the anterior antennæ "Trilobites might then be regarded as original Entomostraca, to be derived from the same racial form as the Phyllopoda." He says, further,[2] "Xiphosura, Hemiaspidæ, and Gigantostraca are themselves again perhaps racially connected with the Trilobites. In any case, however, in the present state of science, it seems probable that all these groups are only connected at their roots with the Crustacea."

[1] Text Book of Comparative Anatomy, English Ed. 1891, p. 415.
[2] *Loc. cit.* p. 421.

From the palæontological record I am essentially in accord with this view, but I am not yet prepared to abandon the position taken in 1881, that all these groups should be arranged under one class and not as an appendage to the Crustacea, as proposed by Dr. Lang. I would go still further, and form a class of the Trilobita and one of the Merostomata.

Two general facts lead me to think that the modern Crustacean is descendant from the Phyllopod branch, and the Trilobita from a distinct branch.[1] 1st. The Trilobite branch exhausted its initial vital energy in Palæozoic time and disappeared. 2nd. The Phyllopod branch developed slowly until after the Trilobita passed its maximum, and then began its great differentiation that approaches culmination in recent times.

When the Trilobite and Phyllopod diverged from their common ancestral Crustacean the Trilobite began at once to differentiate and to use its initial vital energy in developing new species, genera and families. Probably two thousand species and one hundred or more genera are known from the Palæozoic strata. With this great differentiation the initial vital energy was impaired, and the Trilobita died out at the close of Palæozoic time.

The Phyllopod branch continued with little variation until after the Trilobite passed its maximum, and then began to differentiate, until to-day its descendants form the class Crustacea, that corresponds to the class Trilobita in Palæozoic time. Springing from a common Crustacean base the two groups have many features in common, and in carrying out of details of structure in the limbs and gills many striking resemblances occur. It does not impress me that Trilobites were true Entomostracans or Malacostracans; they have certain characteristics in common, but these are not necessarily the result of lineal descent one from the other but are the result of descent from a common ancestral Crustacean type of pre-Cambrian time that lived in the pelagic fauna, in which all the earlier types of life were probably developed[2] and from which, as time passed on, additions must have been made to the palæontologic record of geologic time. The Phyllopoda, Ostracoda, and Trilobita are clearly differentiated in the Lower Cambrian fauna. Bernard is confident that the Trilobites may take a firm place at the root of the Crustacean system, with the existing *Apus* as their nearest ally.[3]

There is much yet to be learned from the study of *Triarthrus* A great amount of material can be readily collected at the locality near Rome, N.Y. It is also of interest to note that the locality at Trenton Falls, N.Y., from which the specimens of *Calymene* and *Ceraurus* were obtained, is only seventeen miles from the Rome

[1] This view is only confirmatory of the result of the profound study of the Apodidæ by Bernard. (The Apodidæ; Nature Series, 1892.)

[2] See Brooks' beautiful Memoir on Salpa, with its suggestive theory of the origin of the bottom faunas of the ocean and the early geologic faunas. The Genus Salpa, Memoirs from the Biological Laboratory of the Johns Hopkins University, ii. 1893, pp. 140–177.

[3] "Nature," 1893, vol. xlviii. p. 582.

locality; that both occur within the Ordovician; and that the stratigraphic position of the bed at Rome is between six and seven hundred feet above that at Trenton Falls.[1]

EXPLANATION OF PLATE VIII.

FIG. 1.—*Triarthrus Becki* (× 3). Outline of Carapace, with appendages represented as they occur on several specimens, their relative position being retained.

a, *a*, *a*, *a*. Endopodites of limbs showing variation in joints.
b, *b*. Plumose portion of exopodite.
c, *c*. The outer or supporting portion of the setæ or fimbriæ of *b*, *b*.
d. Limbs extending from beneath the pygidium, showing large proximal joints. Those of the left side are imperfectly preserved.
e. Antenna extending back nearly to the postero-lateral margin of the hypostoma.
f. One of the cephalic limbs. The basal joint may be broken away on the inner side.
g. Cephalic limb.

FIG. 2 (× 7).—Limbs attached to the under surface of an individual preserving 13 thoracic segments and the pygidium. The limbs (*a* to *k*) on the left side are mainly in place. A fracture cuts out one limb between *g* and *h*.

a to *g*. Limbs preserving traces of the enlarged proximal joints.
b, *d*. Limbs preserving the two joints of the protopodite and two of the large proximal joints.
l, *m*, *o*. Exopodites, showing under or side views.
n. Enlargement of fimbriæ of *m*.
r, *s*. Distal joints of endopodites of right side.
q. Portion of an exopodite showing its inner support.

FIG. 3.—Limbs occurring on the under side of an individual of 14 thoracic segments.

a, *b*, *c*, *d*. Limbs with flattened, enlarged proximal, and slender distal joints.
a. Limb preserving large joints of protopodite (*p*), four enlarged proximal joints and three slender distal joints. At *x* the point of attachment of an exopodite is shown, and in the specimen it looks as though *f* had been broken away from *x*.

FIG. 4.—Restoration of the thoracic limbs of the fifth segment anterior to the pygidium.

en. Endopodite. p. protopodite. a. four proximal swimming joints. b. three distal joints.
ex. Exopodite, attached to same joint of the protopodite as the endopodite.

FIG. 5.—Restoration of the thoracic limbs of the fourth thoracic segment posterior to the head.

en. Endopodite. *ex*. exopodite.

FIG. 6.—Diagrammatic restoration of the second thoracic limb (after Beecher).

FIG. 7.—Restoration of thoracic limb of *Calymene senaria*.

en. Endopodite. *ex*. exopodite. *ep*. epipodite. (Bull. Mus. Comp. Zool. vol. viii. 1881.)

FIG. 8.—Cephalic limb of Calymene × 3; supposed antennule.

FIG. 9.—Cephalic limb figured by Dr. Henry Woodward. (Quart. Journ. Geol. Soc. London, 1870, vol. xxvi. p. 487.) a. Side of hypostoma.

FIG. 10.—Slender jointed legs associated in same beds with Calymene at Cincinnati, Ohio.

[1] The appendages of *Triarthrus* are replaced by iron pyrites, and are usually well preserved. The specimens of Calymene and Ceraurus from the Trenton Limestone of Trenton Falls, N.Y., were replaced by calcite, and in them there were preserved even more delicate parts than I have yet observed in *Triarthrus*. Thin sections were made of the latter and photographs obtained by transmitted light, that were used in illustrating the paper in the Bulletin of the Museum of Comparative Zoology, vol. viii. 1881.

III.—The Basic Eruptive Rocks of Gran (Norway) and their Interpretation. A Criticism.

By H. J. Johnston-Lavis, M.D., F.G.S., etc.;
Prof. (pareg.) of Vulcanology in the R. Univ. of Naples.

BEING present at the reading of the abstract of my friend Prof. Brögger's paper at the last meeting of the British Association, and having listened to certain laudations thereon, I, with several others, went away with the profound conviction that our Scandinavian colleague was about to bring down upon us a mass of evidence bearing on the differentiation of rocks as irresistible and overwhelming as that of his Viking ancestors in times gone by. Since then, however, calmly reading over his admirable memoir recently published in the Quart. Journ. Geol. Soc.,[1] what was my astonishment to find that this veritable avalanche of facts brought forward to support a preconceived hypothesis is of inestimable value as material evidence to be used against the theory of internal chemical segregation, as maintained by that author.

We have described to us several comparatively small eruptive plugs of varying shape the material of which has forced its way through Silurian and Devonian shales and limestones; and considering their limited dimensions has produced intense and widespread contact metamorphism in the country rocks around them by the formation of minerals which Prof. Brögger admits in some cases to have derived part of their constituents from the metamorphosing magma; yet my friend asks us to believe that all the varieties of composition in the igneous rocks are due to chemical segregation. We are told likewise that as we examine the eruptives from north to south, we find them become less and less basic, and this is given as evidence of segregation in the magma taking place in horizontal extension; yet nothing is said of the variations in the composition of the country rock in the same direction.

In the next place the nuclear part of these eruptive stumps is shown to be less basic than the peripheral. This is just what we should expect, and corresponds with the arrangement as displayed in many other dissections of ancient volcanic districts. Prof. Brögger would have us believe that this shows the segregation of a more basic paste near the cooler walls of the cavity, and the concentration of the acid portion in the central and hotter part of the reservoir. Now these facts can be interpreted in quite a different manner. Have we not here strong evidence of the *osmotic* theory, which I maintain is the true general explanation of the variation in eruptive rock composition. A small conduit of an unknown original chemical constituent has, in passing through basic sedimentary rocks, produced in these an intense physico-chemical change and extensive development of new minerals at the expense of the loss of silica, alumina, alkalies and probably other constituents, whilst a very marked gain has taken place with regard to the amount of magnesia and lime. Without an elaborate series of analyses of the alteration products in the surrounding rocks, we can only form a most inaccurate idea

[1] Vol. L. part 1, February, 1894, pp. 15–37.

of where, with the exception of the MgO and CaO, there has really been loss or gain.

Prof. Brögger tells us that the earlier rocks were the more basic, but he hardly gives us any *segregation* explanation why this should be. The *osmotic* theory affords very efficient reason. The first part of an acid magma penetrating limestone, or other basic strata, comes in contact with fresh unaltered rock, and can soon become basified; but in so doing it has formed a practically neutralized lining to the igneous conduit, and consequently the paste that follows will be gradually less and less affected. This earlier basified rock has partly or entirely cooled—in some cases by a re-extension upwards of the igneous matter—this cooled rock is only cracked and reinjected by the more acid paste that follows, just as Prof. Brögger describes as so common at Gran. In other cases the fissure-filling fluid material at a later date extends and breaks the earlier basic rock, enveloping it just as my Scandinavian friend describes the bostonites cutting the comptonites and enveloping rounded, and therefore to me partly eaten up, fragments of the latter, and also pieces of pyroxenite which we are told were due to "earlier crystallizations of basic composition in a paste, from which the distinct magma of the bostonite was not yet separated as a final product of differentiation."

Now how much simpler and more natural is my own explanation of the state of things as given above. Curiously, that very same morning, at Nottingham, when Prof. Brögger read his note, Prof. Sollas brought forward an admirable example of an analogous case of extraneous rock enclosures, and gave the right interpretation of it.

In the bostonite breccias the included fragments of shale are described by Prof. Brögger as being "surrounded by a basic green mass, partly of comptonitic composition," and we are expected to believe that the cooling effects of these fragments caused a more basic segregation on their surface. This phenomenon is of every-day occurrence, and well known to a geologist accustomed to active volcanoes; but so far from favouring the theory of *chemical diffusion and segregation*, it is a striking example of the contact metamorphism of a magma by its solid enclosure. As a rule, the smaller the enveloped fragment, the relatively greater is the basic shell around it; but were the result due to the cooling effect of the enclosure, the larger this was the broader should be the segregation band.

Certainly the analyses given by Prof. Brögger prove in a most striking manner the acquisition of MgO and CaO of the comptonite paste with the actual or relative loss of FeO, Al_2O_3 and the alkalies from the bostonite paste.

Another fact struck me as very remarkable: the two bosses of Brandberget and Solvesberget are smaller and more basic than that of Viksjeldene, and this again is more basic than its larger rival at Dignæs. This is just what we should expect, that the less the size of the injection the greater would be the relative chemical change produced in it by the surrounding sedimentary rocks.

I am glad Prof. Brögger confirms what I have always maintained to be a general principle, that the larger the mass of any given

intrusive rock of a given sized grain, the greater will be the amount of contact metamorphism; also his observation is a most valuable one concerning the difference in the type of such change in the surrounding rocks, as the composition of the acting magma varies.

Looking at this valuable evidence so lucidly worked out by Prof. Brögger in the light of no preconceived idea, we find that it *does not* in any way prove "that differentiation of the original magma has, to an essential degree, been dependent on the laws which govern the sequence of crystallization," but that it does afford the strongest evidence in favour of the differentiation of an original unique paste by the *osmotic reciprocal reaction* between it and that of the country rock that it has been intruded into.[1]

I am satisfied that when these rocks are studied in this new light and in conjunction with other eruptives of the district, it is not improbable that we shall find they are all modifications of one original paste (not that suggested by Prof. Brögger) which *possibly* might be that of the great mass of granitite of the region.

Naples, *March 16th*, 1894.

IV.—On a New Species of *Diplacanthus*, with Remarks on the Acanthodian Shoulder-Girdle.

By R. H. Traquair, M.D., F.R.S., F.G.S.

MANY years ago, and before I entered on my duties in the Edinburgh Museum, the late Mr. C. W. Peach pointed out to me a specimen in the Hugh Miller Collection, which he was inclined to consider as new. It was a Cromarty nodule with dislocated remains, including a dorsal and a pectoral spine, of what was apparently a *Diplacanthus* of unusual size.

I did not consider at the time that the evidence was quite sufficient to warrant the description of this specimen under a new specific name; but since that time, and indeed comparatively recently, specimens have been added to the collection which fully justify Mr. Peach's surmise. Certainly they belong to an undescribed species, and this species is undoubtedly the same as that in the Hugh Miller Collection already referred to. Two of them were purchased two years ago from Mr. Damon, while one occurred in the Powrie Collection; all three are from Gamrie.

This species attains a greater size than *D. striatus*, Ag., the largest entire specimen measuring $7\frac{1}{2}$ inches in length, while still greater dimensions are indicated by the spines of the example in the Hugh Miller Collection. The great distinguishing peculiarity is to be found in the external sculpture of the spines. The longitudinal ridges are fewer and broader, the intervening furrows narrower, than in *D. striatus*, and the flattened surfaces of these ridges are *again finely striated*. On the posterior dorsal spine only two lateral furrows are seen, by which two ridges are marked off in front, the space remaining behind being nearly equal to both of these.

[1] See my paper, "The Causes of the Variation in the Composition of Igneous Rocks," Nat. Science, vol. iv. Feb. 1894.

The pectoral spine is well shown in the Hugh Miller specimen, and has the same sickle-shaped contour as that in *D. striatus*; but the ridges are, as in the case of the dorsal spines, less numerous and more flattened. The secondary striation is not observable on this spine, but it is most distinctly seen on the anterior dorsal one, which lies close to it in the same nodule. The posterior or concave border of the pectoral spine is furnished with recurved denticles, a character also found in *D. striatus*, though not noticed by either Agassiz or Smith Woodward.

The spines seem rather smaller in proportion to the dimensions of the fish than in *D. striatus*. The scales are quite similar in character.

I propose to name this species *Diplacanthus tenuistriatus*, and its recognition is certainly not complicated by any claim to revival on the part of any of the cast-aside synonyms of *D. striatus*.

Formation and Locality.—Old Red Sandstone of the Orcadian series; Cromarty and Gamrie.

Remarks on the Acanthodian Shoulder-Girdle.

It has long been well known that the pectoral spine in *Acanthodes* was supported by a peculiar bone constricted in the middle, expanded at both extremities, and perforated along its long axis by an internal hollow or canal. This ossicle has usually been interpreted as an element of the shoulder-girdle, and designated "clavicle," or "coracoid."

Mr. Smith Woodward, however, in the second part of his "Catalogue of the Fossil Fishes in the British Museum," published in 1891, put forward an entirely different theory as to the nature of this bone. Instead of considering the bone in question to belong to the shoulder, he looks upon it as a "basal" element of the fin. In fact, as I understand the case, Mr. Woodward regards the pectoral fin of *Acanthodes* as specialized from the Selachian type by the abbreviation of the Basipterygium, and finally by the disappearance of all the basals save one.

The configuration of this element in *Acanthodes* certainly suggests strongly the idea of a bone, preformed in cartilage and ossified from the outside inwards, and leaving a conically constricted internal cavity. But there are other Acanthodian genera than *Acanthodes*, and in some of these, notably among the Diplacanthidæ, the form of the bone in question is considerably different. In *Diplacanthus* the bone does not present that hour-glass sort of shape we are familiar with in *Acanthodes*, but is pointed above, expanded and flattened below, so as to assume an appearance reminding us of a Palæoniscid clavicle. And in fact Mr. Woodward does interpret this element in *Diplacanthus* as a "clavicle," and has pointed out two small "infra-clavicular" pieces which connect in the middle line the lower extremities of the clavicles of the two sides. Moreover, he makes the possession, or not, of a clavicle into a taxonomic character: thus the Acanthodidæ have one dorsal fin and no clavicles; the Ischnacanthidæ two dorsal fins and no clavicles; the Diplacan-

thidæ two dorsal fins and clavicles present. Of course in the Acanthodidæ and Ischnacanthidæ the ossicle supporting the pectoral spine is interpreted as a basal piece.

Against this view of Mr. Smith Woodward I protested in a review of his work which appeared in this MAGAZINE for March 1891, and I am still as strongly as ever of opinion that the element which Mr. Woodward calls "clavicle" in *Diplacanthus* is identical with that which he considers as "basal cartilage" in *Acanthodes* and *Ischnacanthus*. And I base this opinion upon the fact that the relation of the supposed basal bone, in the one case, to the pectoral spine, is identical with that of the supposed clavicle in the other; while we may also, in the series of Acanthodian genera, trace every gradation from the most to the least claviculoid shape of the bone.

No one can doubt that the Diplacanthidæ show a more archaic type of configuration than the Acanthodidæ, and among the former the most archaic form is *Climatius*. Here the ossicle in question greatly resembles the vertical limb or upper part of a palæoniscid clavicle, being both thin and flattened, and I have not been able to discover in the specimens before me any trace of the internal tubular hollow seen in *Acanthodes*.[1] In front of the lower end there is an infra-clavicular element.

So there is also in *Parexus*, but in this genus the internal hollow has appeared in the supporting ossicle of the spine, which, though still expanded and flattened below, has become straighter and more cylindrical above, thus showing a decided step towards the "basal" bone of *Acanthodes*. And, indeed, as "basal cartilage" this very element is designated in Mr. Woodward's diagram of the shoulder of *Parexus* (*op. cit.* p. 35), while the piece on the same figure which he designates "clavicle" seems to me, from its position with respect to the anterior extremity of the spine, and also the other element just referred to, infra-clavicular in its nature. This view receives further confirmation from the fact mentioned by Mr. Woodward that these supposed clavicles meet in the middle line. In *Diplacanthus* the supporting bone of the great pectoral spine becomes again very claviculoid in shape, but the internal tubular hollow is there all the same, a fact noticed by Mr. Woodward himself (*op. cit.* p. 23). A pair of infra-clavicular elements is also seen, as already noticed, and these help to support the lesser pectoral spines.

Concerning the relations of the two pectoral spines of this genus, outer and inner, Mr. Woodward says that their axes "are inclined towards one another, and at their proximal extremity they are firmly united by a mass of hard tissue, which is probably to be regarded as the basipterygium or basal cartilage." This is a view with which I cannot agree. In the first place, even if the tissue in question were cartilage, I do not see any resemblance between it and the ossicle, which the author interprets as "basal" cartilage, in *Acanthodes*. But the appearance is, to my eye, solely due to an expansion

[1] Since the above has been in type, I have seen in specimens of *Climatius*, preserved in the British Museum, clear evidence that in this genus the bone in question is furnished with an internal tubular hollow, as in other Acanthodei.

of the bases of the two spines, the portion belonging to each being in one of my specimens apparently divided from the other by a suture; though in others no such division can be seen.

As regards *Ischnacanthus*, it seems to me that no one can look at its shoulder without being convinced that the ossicle supporting its pectoral spine is perfectly homologous with the corresponding element in *Climatius*, *Euthacanthus*, *Parexus* and *Diplacanthus* on the one hand, and *Mesacanthus*, *Cheiracanthus* and *Acanthodes* on the other. Its upper extremity has become more cylindrical, its lower is still considerably expanded.

The shoulder-bone of *Mesacanthus* is still to some extent claviculoid in appearance, the lower half having a laminar expansion, but in *Cheiracanthus* and *Acanthodes* it has become almost quite cylindrical, save just at the lower extremity, where it joins the pectoral spine. Here I may repeat that the relation of this element to the pectoral spine all through the series of genera is so identical that for my part I cannot conceive that the bone should be in one case a "basal," in the other a "clavicle." Therefore I can also see no reason why *Ischnacanthus* should be separated and put into a family apart from the other Diplacanthidæ.

But what *is* the shoulder-bone after all? It cannot well be a basal bone, as microscopically it is, according to Reis, like the spines and scales, entirely composed of Dentine,[1] and its configuration, and association with another plate-like "infra-clavicular" element in certain Diplacanthidæ, pretty certainly indicate that it is also, like the spines, entirely superficial in its origin. I therefore must concur with Reis in considering this bone to be a "claviculoid," or a dermal structure assuming the position and functions of a clavicle.

V.—The Most Recent Changes of Level and their Teaching. Part I. The Raised Beaches.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

THERE are signs accumulating everywhere that the views so logically pressed to their conclusion by Hutton and Playfair, and by a great catena of geologists, since the appearance of the first edition of Lyell's "Principles of Geology," have received a certain check; and no one can read the works of the great Continental geologists without seeing that there is a tendency to reconsider the position, and to hark back to the views of another school of teachers.

While conceding that diurnal and slowly operating causes have done much to smooth and shape the superficial features of the world, many sober geologists are beginning to realize that it is to

[1] In the substance of this bone there seem to be no vascular canals. Reis (Zur Kenntniss des Skelets der Acanthodinen. Geognostische Jahrbücher, 1890, pp. 27 *et seq.*) describes and figures dentine tubules, but these were not observed by Fritsch, who describes the structure as consisting entirely of closely superimposed laminæ (Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Bd. iii. Heft 2, pp. 54, 55).

intermittent and not to continuous and uniform conditions and forces we must attribute its greater features, its mountains, and its ocean depths, its valleys and its scarped cliffs, its chasms and its splintered pyramids, and needles of naked rock.

I know of no more elementary fact in this behalf than that presented by our own islands. That they have been subjected at different times to vast movements of upheaval and subsidence is as plain as the presence of the sun in a July sky; yet if we candidly question the evidence as to what has occurred here in historic times, which means since the Romans planted themselves here, we shall fail to find (at least I have after a diligent search failed to find) any satisfactory evidence whatever to justify the conclusion that there has been a change of level, or even any material change of outline in these islands during the interval.

No doubt there have been local invasions of the coast in Norfolk and Suffolk and Yorkshire, as there have been depositions of silt and mud in the Wash and in the marshy country round it. The Wantsum has been silted up, and the Roman ports of Richborough and Reculver are no longer ports. There have been small local invasions of the coast at Rye and Winchilsea, and there has been a corresponding accretion in Romney Marsh. The two processes have, in fact, compensated each other. Erosion of the projecting headlands and of the land exposed to rough tides has been correlative with the deposition of the products of erosion elsewhere. All this is familiar and elementary.

On the other hand, the continued existence of the Pharos at Dover Castle and the continuance until recent years of the corresponding Pharos of Caligula, at Boulogne, shows that the outline and limits of the coast at these critical points, where the rush of the Channel tide is greatest, must be very much what they were in Roman times; nor is there anywhere, so far as I know, either in the North of England or the South, satisfactory evidence that the land has risen or the sea has sunk since Roman times; while there is some evidence, like that of the Hythe canal, the terminating portions of the Roman wall in the north, and the facts collected on the Cheshire coast, to show that the respective levels of land and sea have remained unaltered.

While I am on this subject I am tempted to quote a not too familiar passage from Hutton, with whom I cannot often agree. "The description," he says, "which Polybius has given of the Euxine, with the two opposite Bosphori, the Mæotis, the Propontis, and the port of Byzantium, are as applicable to the state of things now as they were at the writing of that history. The Isthmus of Corinth is apparently the same at present as it was two or three thousand years ago. Scylla and Charybdis are still, as in ancient times, rocks hazardous for coasting vessels; the Port of Syracuse, with the Island which forms the Greater and Lesser Port, and the Fountain of Arethusa, the water of which the ancients divided from the sea by a wall, do not seem to be altered . . . . on the Coast of Egypt we find the rock on which was built the famous Tower of

Pharos, and at the eastern extremity of the Port of Ermoste the Sea Bath cut in the solid rock upon the shore. Both these rocks are to all appearances the same at this day as they were in ancient times." Similarly Murchison showed that on the Danube the remains of Roman buildings prove that no change of level has taken place since Roman times.

These remarkable facts are proofs, no doubt, of uniformity of continuous conditions having subsisted during the last eighteen centuries; but they are at the same time equally clear proofs that there was a time, or rather there have been many times, when the crust of the earth in these latitudes had more "ups and downs" than it has had recently.

Let us now proceed a step further. If we traverse the limits of recorded history and try to discover what archæological or geological evidence we can reach of the latest changes in the distribution of land and water within our four seas, we are remitted to two kinds of evidence, that of the submerged forests on the one hand, and that of the raised beaches and their associated phenomena on the other. With the submerged forests I do not at present propose to deal, but will limit myself to the raised beaches. What then is the meaning of the raised beaches?

Before we attack the main point, I wish to say something of a subordinate issue, upon which I think a great deal of baseless inference has been built. There are two places in Britain where primitive boulders occur, in which it is possible that they are quite accidental. One is on the coast of Sussex, and the other is the case of the so-called Norwegian boulders in Eastern England. Upon both of these occurrences, as we have said, very large conclusions have been based; and they would, in a measure, be justified, if these boulders were erratics, but on this point there is the gravest doubt. First, in regard to those found in the English Channel (and I am speaking of the foreign stones only) whose provénance has been much debated and in regard to which extraordinary postulates as to shore-ice and floating-ice in the English Channel have been forthcoming, in spite of the fact that the shells and other *débris* found there show *no traces* of great cold, but on the contrary are mixed with Lusitanian and southern forms. It seems to me that not only the very local distribution of these boulders, but the place where they occur, in a reach of embayed waters, are strongly against their having been regular erratics. If they had been so, assuredly we should have found others in the Isle of Wight and on the coast of the mainland east and west of it, besides the mere cluster to which I refer, and which has been so much discussed. How, then, would I account for these boulders? By the simple theory that they may be ballast which has either been thrown overboard or has come from some ship which has gone to pieces on the coast. Assuredly this is a very reasonable explanation of the facts and much more in accordance with scientific laws of evidence than the current theory, and it ought to be discussed and its possibility tested before we are called upon to face much more difficult causes.

I would apply precisely the same explanation to the so-called Scandinavian boulders of Eastern England; these boulders have been the mainstay of those who have argued in favour of a North Sea ice-sheet, which is supposed to have filled up the whole area between Scandinavia and Britain.

In my recent book on the Glacial Nightmare, I have advanced a number of arguments against this theory. Professor Bonney has recently added a new and striking one, based upon the difficulty such an ice-sheet would have in traversing the hollow trough which runs down the western coast of Scandinavia; these do not, however, exhaust all the arguments. If a portentous ice-sheet, loaded with various kinds of stone, had come all the way from Scandinavia to Britain, we should expect to find traces of its terminal moraine, and we should certainly expect to find not only a few scattered stones on the beach in Yorkshire or East Anglia, but large numbers of them inland, not only in England, but more especially in Scotland. It must be remembered also that the most characteristic of these stones, if not all of them, come not from the part of Norway opposite Britain, but the Christiania Fjord. This is very emphatically maintained by the late Prof. Carvill Lewis and the Northern geologists, whence it is most difficult to see how an ice-sheet could have travelled to Britain at all.

The fact is, the whole theory of a North Sea ice-sheet, in so far as it is based on these boulders, is a stupendous test and trial of human credulity. I believe these stones to be like those from the English Channel, merely ballast which has been thrown overboard, or left by some wrecked ships. I may say that my very acute friend Professor Hughes, of Cambridge, shares this opinion, and since I wrote this has published his views in "Nature," which I feel to be a very important support to my contention. He tells me that he once entered the hull of a wrecked ship on the coast of East Anglia which was being broken by the waves, and which contained in its hold ballast consisting of foreign primitive rocks. This seems to me to be a very useful instance of an occurrence which must have been very frequent indeed on this much exposed coast, and which furnishes a reasonable key to a problem which has mystified many people, and grateful to those who believe in sobriety as a potent factor in scientific reasoning, and who have not been led captive by the charming but delusive rhetoric of Agassiz and his scholars.

Having discussed the interfering factor involved in the foreign stones on the Yorkshire and Hampshire coast, let us now turn more directly to the meaning of the raised beaches.

They are generally taken to mean that the beaches represent the level at which the sea once stood, and we are asked to concede that these beaches prove a former submergence of the land up to the point where they occur. That they do mean this is clear enough, but it seems to me that this is not all. The really interesting question is not whether the sea once reached up to the level of these beaches, but whether its presence there was lasting and continued or merely transient and ephemeral. Do the beaches mark

the result of a more or less permanent change of level in the sea margin, by which the land was upheaved, or do they merely mark the highest level of a transient wave or rush of water, such as is left by every high tide of the sea, and by every flooded river, or rushing current escaping from a lake which has burst its barriers, along the margin of its highest rush.

The former view is that which chiefly prevails among the teachers of Uniformity, who prefer to make whole continents rise and fall to explain every similar phenomenon, so long as the process is a slow one, than have recourse to any unusual displacement of water on a large scale, which must follow any such upheaval or subsidence on a much smaller scale when rapid. I must confess to being a supporter of the latter view.

In the first place I would call attention to the exceedingly sporadic and scattered character of these raised beaches. They are nowhere continuous for any distance; they consist chiefly of mere shreds and patches of gravel here and there, and not of continuous and deep shingle beaches and masses of sand with marine shells in them, such as the margins of the ocean would deposit. If there had been a continuous and widely-spread elevation of the land we should certainly have had in many places a replica of the modern strands. Not only so, but these strands would have passed continuously round all the inlets and recessed gulfs and bays of the land, where they would have been virtually proof against subsequent denudation.

Again, the sea would in this case have invaded all the lowlands within its reach, and would in that case have covered them with unmistakable marine deposits, mud and sand with marine *débris*. Of evidence of this I can find none. This applies to the low-level beaches. If we turn to the high-level ones the case is even more remarkable. When we remember the height at which the Moel Tryfaen and the Macclesfield beaches occur, it seems impossible to understand, if they mark an actual prolonged submergence to that extent, how it is that over large areas in England we do not find most unmistakable evidence of prolonged submergence in the shape of unmistakable marine deposits with the manifold wreckage of the sea, for the submergence in question must have covered a large part of these islands with a great depth of water, and according to Uniformitarian views must have done so for a long period.

I cannot, therefore, see in these beaches the proof of prolonged submergence which others profess to see, but rather the passage along the shore of a great tidal wave or rush of water, caused by disturbance of the strata somewhere, which took up a certain quantity of gravel, etc., and left it at its own high water-mark, at once a proof of its presence, and a measure of its intensity at the spot of the wave in question.

The next, and a very important, point to remember, which has been much overlooked, is the irregular height at which these raised beaches occur. If they had been due to the general upheaval of the land, we should have found assuredly some common level among them; instead of this it is scarcely possible to find any two of them

at the same height. In inlets and coves they seem to rise as we go inland; elsewhere their elevation is as irregular and sloping as that of the terraces in some of the Scotch inlets is. This seems consistent not with a general and widespread elevation of the land, but with the rush of a mass of water, which when throttled in a narrow passage would rise to a higher level, and when it had free elbow room would spread out, just in the same way that we can see the Race in the Bristol Channel and in the Gulf of St. Micael spread out. Hence it does not seem to me that we have much good warranty for always separating the high- and the low-level beaches. They are possibly, if not probably, due to the same impulse and the same movement, and merely mark its varying phases, whether it was constricted or had room to flow freely. When it met with a barrier and its flow was rapid it would rise; hence, probably, the reason for the height of the beach at Moel Tryfaen. Where it had room to spread it doubtless reached far into the land until it spent itself. Hence, perhaps, the explanation of the occurrence of Whale's bones and skeletons in the low-lying districts south of the Wash, in Cambridgeshire, and the shells found in the district called the Straits of Malvern, which were once probably overrun by a great Race from the Bristol Channel. I take this to be the most rational explanation of the raised beaches and of the existence of marine *débris* in certain very local inland districts, which it has been the custom in so many geological manuals to quote as unmistakable witnesses of a general change of level of land and sea where they occur. I would extend this argument to all the cases where there is no clear evidence of the land being in an area of proved upheaval, as in Northern Scandinavia, in Greenland, and in Labrador. I would extend it also to the shell beds such as those at Uddevalla, which no stretch of the imagination would attribute to an old beach, and where I have spent many hours among them; also to some of the so-called raised beaches of Canada. I would of course except some of those cases where the shells have actually been found as they lived; but even here the greatest caution is necessary in interpreting the facts, for shells with their valves united and enclosed in clay might, under many conditions, be removed with the clay *en masse*, and thus simulate beds *in situ*. Within our seas I know of no raised beaches which can unmistakably claim to mark old sea-levels, but only the levels of one or more great cataclysmic tide or tides or waves—perhaps the very same wave or waves which spread out the drift and mixed the Boulder-clay and the gravel and sand of the Eskers with fragments of marine shells, etc.

Another question, of course, remains, and one which is very difficult of solution, namely, as to whether the various raised beaches at different levels which occur round these islands are the results of one considerable wave or of several, occurring at different times. It seems reasonable, at all events, to suppose that the low-level beaches and the high-level beaches belong to different diluvial movements. We have some evidence that one such movement took place in Northern Europe in the second century B.C. in the so-called Cimbric

flood, mentioned by Florus as the cause of the migration of the Cimbri and the Teutones. The occurrence of the remains of a boat in the 20-foot beach on the Clyde points to one of these movements at least having been much later than the diluvial movement which destroyed the Mammoth and its companions. Perhaps the deposition of this beach was coincident with the so-called Cimbric flood.

These are questions, however, which the future must settle; what we are now chiefly constrained to urge is, that the raised beaches which have been supposed to index immense movements of the earth's crust, up and down, are capable of a much more moderate and much more plausible explanation, namely, that they are necklaces put about the land by the amorous sea in one of its more ardent moments, and correspond to the line of wreckage in the fields after an inundation.

VI.—The Corrugation of the Earth's Surface and Volcanic Phenomena.[1]

By Arthur Vaughan, B.A., B.Sc.

IT is important, in the first place, to discuss those facts concerning the Earth which must be taken into account in any attempt to deal satisfactorily with the problem before us.

To a depth of about 80 feet in Temperate regions, the earth has its temperature determined by the seasons; being warmed in summer and cooled in winter. Below this average depth the temperature remains constant throughout the year, and shows a steady increase as we go downwards. By observations made in deep borings, it is found that this increase is at the rate of about 1° F. for every 60 feet, and that this law holds true throughout a descent of several hundred feet; but that the rate of increase diminishes at considerable depths. This increase in temperature with distance from the surface points unmistakably to a large store of heat in the interior of the earth, and the increase in the rate at which temperature diminishes, as we approach the surface, agrees with what we should expect to happen if a large heated mass were allowed to cool; the nearer the surface the greater would be the rate of loss of heat, and consequently the more rapid the diminution of temperature.

The next important point to be considered is that, however the rocks near the surface may differ in composition, they are all distinguished by being divided up into larger or smaller portions by joints, bedding-plane, etc. The actual amount of separation caused by this means varies very much with different rocks; but in so far as it exists must necessarily allow of closer approximation if the rocks ever become subjected to great pressure. That this jointing is sufficient to allow of seasonal expansion is obvious, notwithstanding the fact that the difference between summer and winter temperature in continental regions is often extreme. Again, this division into smaller portions by jointing, etc., remains true at all

[1] The author wishes it to be stated that this article was sent in on March 31st, 1894, but has not appeared earlier for want of space.—Edit. Geol. Mag.

depths to which observation has been carried; so that this property may be roughly said to define what is meant by the crust.

Let us now carefully consider what would be the effect of allowing a large heated mass to cool. We will first take the simplest case, namely, a sphere of homogeneous material. Considering the mass as made up of consecutive concentric shells, the outermost shell, on account of its greater exposure and closer proximity to the cooling influences, will cool faster than the next inner; but, since cooling necessarily produces contraction, and since the amount of contraction is dependent on the loss of temperature, the external shell will contract faster than that underlying it. It obviously follows that the outer shell exerts a squeezing force upon the interior, and, by compressing the mass into a smaller volume, increases its density. The interior in its turn prevents the outer shell from contracting to the full extent proper to its loss of temperature, and thus sets up a state of strain in the outer shell. Thus, progressively, each shell, by contracting upon the next inner one, increases the density of the interior, and is itself at the same time thrown into a state of strain.

A familiar illustration of this process is the brittleness induced in most metals by rapid cooling, and to minimize this state of strain recourse is had to the process of annealing. This reasoning seems to apply with equal force to the case of the earth. Each shell, as it contracts from loss of heat, compresses the included mass and increases its density; continued contraction of shell upon shell produces cumulative results in the same direction.

From this very effect also the amount of contraction will increase; for it is found that the coefficient of expansion of any material increases with its density. This, perhaps, affords an explanation of the fact that, whilst the specific gravity of the surface rocks averages about 2·5, that of the whole globe reaches 5·5.

It may, perhaps, be diffidently suggested that in a molten mass there would be a tendency to lessen the effect of differential contraction by a rearrangement of material in such a way that each successively smaller shell should have a greater coefficient of expansion to recompense it for a smaller loss of temperature.

Further, the effect of gravity upon such a molten mass would be to cause the accumulation of the heavier material towards the centre.

These two causes—the effect of gravitation on a molten mass and the subsequent effects of contraction—must be looked to as furnishing a probable cause of increased density in the interior.

To the compressing force of contraction we must also look for an explanation of the fact, established by astronomy, that the whole globe is solid. For, judging by the increase of temperature towards the centre, the rocks of the interior must be far above their melting point, and can only be prevented from melting by subjection to great pressure.

It must be doubted whether superincumbent pressure can have any great effect; for, were the whole globe composed of concentric homogeneous ellipsoidal shells, each such shell of gravitating matter must be necessarily self-supportive, and could only exert pressure upon the interior by undergoing contraction.

So far we have reasoned as if the whole globe were made up of homogeneous shells. But the crust is composed, and has been so throughout all geological time of which we have any accurate knowledge, of rocks differing greatly in their capabilities of expansion and contraction; and further the greater part of the crust has its temperature determined by earth-heat, or in other words possesses heat which it must be gradually losing. It is consequently important to obtain some idea of what would occur during the cooling of a large heated mass, where outer shells were composed of materials with different coefficients of expansion.

The first effect, caused by the cooling of the outermost shell, will be to produce a pressure upon the interior; but the strain thus set up in the outer shell will be gradually relieved by the splitting permitted between materials of different powers of contraction.

Thus the outermost shell will consist of a great number of slightly disjointed portions. This process will be continued throughout the cooling of the outer heterogeneous shells. When the interior, supposed homogeneous, begins to contract, the disjointed parts of the outer shell will be brought closer together to accommodate themselves to a smaller area—a movement of which they will always be capable without the formation of folds or rucks, since the loss of temperature is necessarily greater the nearer the surface.

Applying this reasoning to the contraction of the crust of the earth, it seems probable that jointing must ensue in the manner described above in all such rocks as have heat to lose. We do not, however, maintain that this is the only way in which such splitting can be brought about, for, no doubt, the shrinking from loss of moisture has always been a very effective cause.

We will now attempt to obtain some estimate of the amount of jointing necessary in order that the outer shells may follow a contracting interior. Let us assume a shell at the depth of a few miles to have its temperature diminished 10°, and further, let us suppose that this contraction is unresisted by a more slowly contracting interior.

For the purposes of actual calculation let us suppose the material of which the shell is composed to have a coefficient of linear contraction equal to ·00001174 (this is the coefficient of sandstone, which is intermediate between the coefficients for cast and wrought iron, and is greater than that for marble). From these data the contraction of one mile is found to be 7·2 inches. This, then, is the amount by which one mile length of the next overlying layer must be able to contract to avoid rucking up.

Putting this statement into another form; every yard of shell just overlying the contracting interior must be capable of shrinking $\frac{1}{2\frac{1}{4}\frac{1}{0}}$ of an inch to avoid rucking. The amount required at the surface will not appreciably exceed the above amount; for, if we imagine contraction to take place at a depth of five miles, the required amount of shrinking in each yard of arc at the surface will be approximately $\frac{4}{3}\frac{0}{9}\frac{0}{9}\frac{0}{5} \times \frac{1}{2\frac{1}{4}\frac{1}{0}}$ of an inch.

It would seem that there could be no difficulty in assuming sufficient separation space in all known rocks to allow of this extent of approximation. But the case is in reality much stronger than appears in the above figures, for we have assumed no contraction to go on in any of the overlying rocks, whereas contraction must be in progress in all those rocks whose constant temperature exceeds the mean average surface temperature, and this at a faster rate than in the underlying interior, with the result that the separation spaces throughout the overlying shells are being increased so as to allow of additional approximation.

There would seem, then, no difficulty in accepting the idea that the crust will in general follow the contracting interior and not be thrown into folds, but will accommodate itself to a smaller area by the closer approximation of its component parts.

More space has been spent in considering this point than would perhaps seem necessary; but the theory has very generally been accepted that corrugations are caused by the shrinking away of a contracting nucleus from a consolidated non-contracting crust.[1]

It will be well, consequently, to examine what amount of corrugation could be produced on the supposition that the crust is non-contracting and incapable of accommodating itself to a smaller area. The interior is imagined to shrink away, but, by the action of gravity, the heavier areas of the heterogeneous crust are retained in contact with the shrinking nucleus. It follows, on the hypothesis of an incompressible crust, that the intermediate portions will be thrown into folds.

Employing the same data as before, and further assuming that the two areas, which, owing to the effects of gravity, sink with the contracting nucleus, are 1000 miles apart, we find by an easy calculation that the greatest possible height to which the under surface of the intermediate portions of the crust could rise is less than 5000 feet.

This is on the assumption that the crust is bent into two planes meeting above the middle point of the contracting arc. Again, since we cannot suppose the contracting nucleus to lie many miles beneath the surface, this maximum elevation will not be sensibly increased at the surface itself. It is very important to point out the favourable exaggeration involved in the data employed in the above calculation.

In the first place, owing to resistance from within, no shell could contract to its full extent; secondly, a fall of 10° would represent an enormous loss of heat in the interior; again, it is improbable that the two areas which sink with the nucleus could be so far apart; and lastly, that the crust should be thrown into the form supposed is utterly impossible, and at variance with the structure of all known mountain systems.

One further point in regard to this theory :—

Since the effect of the shrinking interior must be to draw closer together those portions of the crust which are in contact with it, the force between any two portions of the earth's crust must be one

[1] A discussion of the later theory of Mr. Mellard Reade is contained in Part II. of this paper.

of compression. Thus any relief, sought for by the breaking of the crust, can only be found in the pushing of one portion over the other; that is to say, in the production of reversed faults. Normal faults could find no place as consequences of such a system of earth movement.

To pass on now to give what we think to be a possible and adequate explanation—

Let us suppose that some large area on the earth's surface is at considerably less distance from the centre than the surrounding regions, and that furthermore the surface rocks in such an area are maintained at a very low temperature. It follows that under such an area the surfaces of equal temperature will be at once more numerous and more crowded than under the surrounding regions—more numerous because, the surface being kept at a lower mean temperature, the range of temperature from the interior to the surface is greater and more crowded because the distance from the centre to any point of our hypothetical area is less than in the surrounding areas. In this case the distance between two points in the same radius, having stated temperatures, will be less than in the regions surrounding the area. In consequence, the transference of heat from one point to the other will be greater; or in other words the loss of heat will be greatest under the depressed area. Thus beneath such an area contraction will proceed at a faster rate.

In this place we wish to point out, once for all, that we shall simply consider the effects of contraction upon the nucleus which we imagine to be practically homogeneous at equal distances from the centre. We shall further assume that the overlying crust, being formed of heterogeneous materials, will follow a sinking interior without any rucking, for the reasons given at length above. This distinction in geneity between crust and nucleus probably means no more than that the materials composing the originally homogeneous surface shells have been rearranged by agencies, with which we are not now concerned, to form a heterogeneous crust. It will further appear, on carefully considering the areas under discussion, that the thickness of the crust may be considered to be at least small.

From the faster contraction which we have shown must take place under such an area, two results necessarily follow. In the first place there will be a great strain upon the rocks bordering upon the depressed area, since the tendency of contraction must necessarily be to draw them closer together. The effect of this strain will necessarily be in the direction of lessening the rapidity of descent from the elevated to the depressed areas, and in this attempt pressure will be produced on the underlying rocks. Secondly, on account of this very resistance to contraction, any arc traced on the area, being in the form of a broad arch, whose centre is approximately the centre of the earth, will in attempting to contract bend inwards so as to lessen its curvature, and consequently shorten its length. In performing such a movement, the underlying

material will be, so to speak, squeezed out, and this will cause a real transfer from under the sinking area to beneath the surrounding regions. We may reasonably expect great compression, contortion, and plication to accompany such a movement. Such transference must cause real elevation of the surrounding regions in addition to the relative elevation due to depression of the sinking districts.

That this theory would be adequate to account for the present height of mountains seems very probable from the following considerations. The simple fact that a large area is depressed, and therefore the sea-level brought nearer the centre of the earth, implies the raising of the undepressed parts above it. Due to this cause, we obtain the same maximum height as was found possible under the theory referred to above.

In the second place, actual transfer of material from under the sinking area must add still more to the elevation caused by relative displacement.

Lastly, we have the important fact that the rate of contraction beneath such areas is far in excess of the average under neighbouring areas, and that this rate is maintained by the continual contraction of fresh portions of the heated interior, brought into contact with the cold outer layers by the squeezing out of intermediate material.

The material thus pressed out from under the contracting shell-caps and forced beneath the surrounding regions, will exert an additional strain upon the overlying crust, which, as explained above, has already been stretched by the contraction itself. In this process it is quite conceivable that great metamorphism will result from the movement under enormous pressure. But, so long as this pressure is unrelieved, no igneous rocks could possibly be generated; for, though no doubt the compression will raise the rocks upon which it acts to a temperature far above their melting point, any increase of volume which melting would imply will be strenuously resisted.

Two extreme cases must now be discussed:—

First: suppose the change from elevated to depressed areas to be spread over a large space and to be uniformly slow. In this case, the strain upon the overlying rocks will be spread over a very large area, and there will be no lines of special weakness. Here, then, we should not expect the crust to split right through at any point, but rather that it would suffer an uniform extension by the extension and development of local separation spaces, and in this way very gradually relieve the state of tension. This gradual relief from pressure might be sufficient to slowly melt the underlying materials, which, when first brought into the position contemplated, must have been under so great a pressure as to be raised above their melting point.

The slowness of the action would ensure the production of a holocrystalline igneous rock.

Secondly: suppose the change from elevated to depressed areas to be very rapid. Here we have special lines of weakness afforded by sudden bends, and the probability seems to be that at such points the crust would be unable to withstand the total strain and would split right through.

The portion nearest the sinking area would thus be drawn away from the rest of the crust, which would be raised to a higher level by the pressure from below. In this way a normal fault, or succession of such faults, would be brought about.

Again, the sudden relief from great pressure caused by this means, when the underlying rocks are far above their melting point, would result in their sudden melting. The very high temperature at which this melting may be supposed to be produced will necessarily ensure very complete liquefaction, and aided partly by the pressure from below, and partly by the increase in volume due to melting itself, the molten material will be exuded through the clefts formed in the broken crust, to rise to the surface in a flow of lava. Further, the rupture of the crust would certainly cause an earthquake, and, as we have shown, it would be natural to expect depression or elevation to be a consequence. That earthquakes are often followed by such phenomena is well known.

Again, the greatest pressure being between the stretched crust and the upward forced interior, the depth at which earthquakes would be generated on this theory seems to accord well with observed results. It merely remains now to point out where such areas as we have considered exist, and to illustrate the effects of the theory by well-known examples. In the first place the earth is flattened at the Poles, so that each Pole is about thirteen miles nearer the centre than any point in the equator. Again, the Polar regions have, for vast ages, been covered by ice, so that the ground is constantly frozen far below the surface; also the change is very gradual from elevated to depressed areas.

We should expect then that the Polar regions themselves are sinking, but that the neighbouring regions are rising, and that this should be an effect spread over large areas. These facts seem to be in accord with the observations that whilst Greenland is found to be sinking, Norway is rising.

A suggestion may here be thrown out as to a possible explanation of the events supposed to have occurred during the Glacial epoch.

Before the Glacial period there is evidence that the British Isles, as a whole, were above sea-level; but that during the period subsidence took place and that, after its departure, the Islands again rose. This seems to be in entire agreement with what the above theory would require. During the period of great cold, lasting for an enormous time, the ground must have been frozen to great depths, as is proved by borings in Siberia, where the effects are still very obvious in frozen ground, extending to several hundred feet. In consequence rapid contraction must have taken place beneath the area, together with the transfer of material from below. These two causes combined must have drawn the area down to below the sea-level. On the termination of the period, the Polar regions must again have become the principal area of subsidence, resulting in the gradual upheaval of the neighbouring regions, as evidenced by the raised beaches of Norway and Scotland which characterized the post-Glacial period.

The second class of areas, which satisfy the conditions of our theory, are the deep ocean floors. Here we have depressed areas surrounded by more elevated regions; the surface layers being maintained at a very low temperature, as determined by recent soundings. These, in general, afford examples of the case in which the passage from elevated to depressed areas is rapid. We should therefore expect to find, along the borders of such areas, evidence of volcanic phenomena. It is sufficient to quote, as illustrative examples, the vast ring of volcanoes which surrounds the Pacific Ocean. It seems also specially worthy of notice that Japan is the scene of continually recurring earthquakes. Now it is on the borders of the Pacific in the neighbourhood of Japan that we find the most rapid passage from elevated to depressed areas, for here is the deepest depression in the whole ocean. In consequence, we have here a line of great weakness, and should expect earthquakes to be most frequent if in any way dependent upon the above explained cause. As a last point of agreement with the theory, we may notice Darwin's theory of Coral reefs as pointing to the comparatively rapid sinking of the deep sea floors.

In conclusion, a few words may be said upon the necessary consequences of the truth of this theory. It points unmistakably to the permanence of continental and deep sea areas, and would indicate that the tendency has always been to deepen and render narrower the ocean basins. Perhaps, also, it points to a continual tendency to render the globe more elliptical by increasing the flattening at the Poles. It would also necessarily follow that great elevations could only take place on the borders of deep oceans, or in proximity to regions of prolonged cold. Lastly, that volcanic phenomena may be expected to occur along the coast regions of great oceans.

It may be remarked, as a necessary deduction from the first of the above statements, that the crust covering the areas with which we have dealt must be extremely thin, and that, at the bottom of the deep sea basins, we should expect to find the nearest possible approach to the primitive crust.

It is, of course, not assumed that this theory is applicable to all cases of subsidence and elevation which, when local, are no doubt due to much less potent forces, such as denudation and deposition.

(*To be continued.*)

VII.—On the alleged Conversion of Chlorite into Biotite by Contact Action.

By Lieutenant-General C. A. McMahon, F.G.S.

I HAVE to thank Dr. Callaway for the courtesy and good-humour with which he has received my criticisms on his paper in the December Number of the Geological Magazine. Would that all controversial fencing were conducted with equally well-tipped foils.

My Roman soldiers, in particular, have been received with a kindness that reminds me of the friendly meetings of the British

and French officers in the Peninsular War during brief seasons of armistice. Indeed, one might almost go so far as to say that friendly intercourse with these Roman cohorts has imbued Dr. Callaway with a taste for military tactics, and has inspired him to perform a masterly change of front under cover of the active advance of a line of skirmishers.

We are now told that the conversion of chlorite into biotite is due to contact action; indeed, Dr. Callaway "thinks it probable that there is not a scrap of biotite in the crystallines of the Malverns which has been produced except by 'contact action.'" I am glad that this fact has been clearly brought out, as I unfortunately received the impression from Dr. Callaway's paper—an erroneous one as it now appears—that the conversion of chlorite into biotite was considered a case of dynamo-metamorphism.

I cannot attribute this misapprehension altogether to my own stupidity. If Dr. Callaway's language had been more precise, and to the point, I should not, I think, have fallen into this error.

My criticism was expressly confined to the four corners of Dr. Callaway's paper. In that paper he did not say that he considered the change of chlorite into biotite a contact phenomenon. He did not state that the heat required to convert a hydrous into an anhydrous mineral was due to the proximity of eruptive granite; on the contrary, he used language that seemed to point directly to the generation of heat by dynamic agencies. We were told that "the temperature of metamorphism . . . often rose to the point of fusion in the *shear zones*"; and further on he wrote, "where the rock is *slightly crushed* [the *italics* are mine], and there are no signs of rock-fusion [the fusion depending apparently on the amount of crushing], there is decomposition of hornblende; but when there is *intense crushing* and *shearing*, accompanied by a high temperature, reconstruction sets in and biotite is generated."

If Dr. Callaway wished to convert petrologists to the belief that chlorite is converted into biotite by *contact* action, I think it was unfortunate that he did not expressly say so, and that he used language that seemed to indicate a belief, on his part, that dynamic action was the cause of the conversion.

Dr. Callaway goes on to remark: "It would therefore appear that General McMahon admits the conversion of chlorite to biotite by contact action; and if so, I want to know why he opposes my theory." In other places he says that he "does not understand my position," and asks me to "elucidate" it. I trust, therefore, that I may be pardoned for entering into a discussion on this point. I shall be very brief.

I did not admit the conversion of chlorite into biotite. What I said was: "In cases of contact action one can readily understand how aqueous acid vapours, or liquids, emanating from the molten igneous rock under high pressure penetrated the adjoining rocks, and carried with them in solution some of the constituents of the igneous magma." It is certainly fair to imply from this sentence that I admit the generation of biotite by contact action; but that

is not quite the same as saying that the secondary dark mica, that owes its genesis to contact action, is chlorite converted *in situ* into biotite.

I may say, in passing, that I do not deny the *possibility* of the conversion of chlorite into biotite; but if this process really does take place it must be a very complex one; and it does not follow from the admission that the thing is *possible*, or even from the further admission that it *may* conceivably take place occasionally, under special circumstances, that I believe this process to be the one usually adopted by nature.

A very important remark was made by Mr. Teall at p. 221 of his British Petrography (see also the footnote at p. 642 of his paper on the Whin Sill in the Q.J.G.S. vol. xl.) when speaking of the introduction of albite into rocks altered by diabase, that the secondary mineral was "due to the actual impregnation of the surrounding sediment by material derived from the eruptive rocks."[1]

That dark mica is an exceedingly common product of contact metamorphism, in basic eruptive rocks altered by granite, is known to all petrologists. I have several very interesting examples of this among my Himalayan specimens, and I have described one in the Records of the Geological Survey of India, vol. xix. pp. 71–77. The acceptance of a dark mica (it need not always be biotite), however, as a contact mineral, does not necessitate the acceptance of Dr. Callaway's theory. We are naturally led to ask, Is not this mica the result of direct impregnation? Must the genesis of contact-mica be preceded by the production of hydrous chlorite? For that is what Dr. Callaway's theory involves.

It would not, of course, convince Dr. Callaway to assert that dark contact-mica may often be found in rocks which do not contain chlorite, because he might allege that chlorite was there in the first instance and that the whole of it had been manufactured into biotite.

Our courts of law, in the difficult task of weighing evidence and of deciding which of two conflicting witnesses is to be believed, are very much guided by considering the *probabilities* of the case. This principle is also a most valuable one in helping scientific men to decide between rival hypotheses. In the present case we are dealing with the contact action of granite. We know that granite must have consolidated at a considerable distance below the surface of the earth; and that, consequently, a long period (measured in geological time) must have intervened between its consolidation and its exposure at the surface. The sinking of that portion of the earth's crust, and the accompanying deposition of new strata, must have ceased; and a long period of elevation and erosion must have set in. This supposition is inevitable if we have to account for the removal of some 20,000 or 30,000 feet of superimposed strata, and the exposal of granite at the earth's surface.

[1] In 1883 I remarked with reference to a Himalayan rock: "these facts appear to me to indicate that the rock was subjected to two different processes of contact metamorphism; one process—due to heat; . . . . whilst the second process was probably the injection of matter from the granite rock, possibly in a gaseous or liquid condition, along lines that followed the original direction of lamination or cleavage." Records, G.S.I. vol. xvi. p. 137.

Whilst this granite was being slowly brought to the surface, the rocks that accompanied it must have been exposed to those potent aqueous agencies which effected so many alterations in igneous rocks, and the traces of which petrologists see in almost every slice of basic rock examined under the microscope.

The Malvern basic rocks cannot have escaped the ravages of those aqueous agencies. Is it not far more probable then that the chlorite found in those metamorphosed rocks was produced by aqueous agencies *after* the formation of the biotite by contact action, than that the chlorite was formed *before* the eruption of the granite; and, like Shadrach, Meshach, and Abed-nego, escaped absolutely unsinged from the burning fiery furnace of contact metamorphism that converted their fellows from hydrous into anhydrous minerals?

I do not think it is necessary to suppose that the chlorite in the Malvern crystalline rocks was derived from biotite; it may have been derived from some of the other minerals in the rocks. But, on the other hand, the conversion of dark mica into chlorite would not, I think, present much difficulty to a petrologist who knows the story of the conversion of olivine and augite into serpentine. The passage of micas, such as biotite and phlogopite, into members of the vermiculite group ("closely related to the chlorites") is recognised by Dana (System, 6th Edn. p. 664). If phlogopite, a magnesian mica "near biotite," can be altered into serpentine (Dana, *ib.* p. 634) there is no difficulty in believing that biotite, or a dark mica allied to that mineral, can be converted, in the wet way, into a mineral of the chlorite, or vermiculite, groups.

In the case of basic rocks containing magnesian minerals, there would be no difficulty in accounting for the accession of additional magnesia—the magnesian minerals undergoing decomposition would supply that.

Dr. Callaway gives 34 per cent. as the average proportion of magnesia in chlorite; and the actual analyses of biotite (*vide* Dana) give, in some cases, proportions as high as 28 per cent., and in the case of phlogopite as high as 29 per cent. Very little, if any, magnesia would therefore be required for the conversion of a mica rich in magnesia into chlorite. I say "if any," because Dr. Callaway's figures confessedly represent the average proportion. In actual analyses it varies greatly and sometimes falls below the average; and further the removal of portions of the other bases (a necessary part of the process) would alter the proportion of magnesia to the bases that remained.

In conclusion I think it desirable, with reference to Dr. Callaway's remarks, to reinsist on the distinction to be drawn between an observed fact and a hypothesis advanced to explain a fact. The fact actually observed is the existence of chlorite and biotite in the same rock. Dr. Callaway explains this by one hypothesis; I explain it by another. I submit that he is not entitled to allege that the conversion of chlorite into biotite is an observed fact—any kaleidoscopic brandishing of authorities before our eyes notwithstanding—until he has shown that the hypothesis he advocates

is the mode actually followed by nature; and that the hypothesis I advocate is not nature's mode of action. I submit that when chlorite and biotite are found together in the same rock, under the circumstances stated by Dr. Callaway, that it is more in harmony with the probabilities of the case to believe that the hydrous chlorite was created by aqueous agencies after the intrusion of the granite than to believe that the chlorite was created before the intrusion; that the biotite was formed out of a portion of the chlorite; and that during the long ages that succeeded, aqueous agencies (contrary to our ordinary experience) either had no access to these rocks, or that (contrary to their known habit) they altogether failed to form any chlorite out of the unstable basic minerals exposed to their ravages.

REVIEWS.

I.—Peter Redpath Museum, McGill University, Montreal, October, 1893. The Canadian Ice-Age; being Notes on the Pleistocene Geology of Canada, with especial reference to the Life of the Period and its Climatal Conditions, and Lists of the Specimens in the Museum. By Sir J. William Dawson, C.M.G., LL.D., F.R.S., F.G.S., etc. 8vo. pp. 301, with 25 Illustrations. (Montreal, William V. Dawson; New York, Scientific Publishing Company. 1893.)

SIR WILLIAM DAWSON states that for thirty years he has aspired to make Canada as typical a region for the study of the Pleistocene period, as Sir William Logan made it typical for the Laurentian rocks. His guiding principles are briefly expressed in convictions that the phenomena of the Glacial period are to be explained by more active and extensive operation of the ordinary causes still existing in more northern regions; that there has not at any time been a Polar ice-cap or great continental ice-sheets; and that the astronomical theory of glacial cold is incapable of fully explaining the facts; but that the cold climate was mainly a result of geographical conditions involving a different distribution of ocean currents, and extreme local evaporation and condensation, giving rise to local glaciers of great volume and to floating ice. It is inferred that the close of the Glacial period cannot have antedated by many centuries or milleniums the first appearance of man as known in history.

The book is divided into seven chapters. The first chapter is chiefly a series of quotations mainly from the author's writings, chosen to illustrate the principles with which he starts.

The second chapter discusses the succession of deposits; which comprise a Lower Boulder-clay, with some travelled boulders and a few Arctic shells; succeeded by the Lower and Upper Leda-clay, with marine shells and drift-plants; and an Upper Boulder-clay, including sand with *Saxicava* and gravel. These deposits are found in the Lower St. Lawrence, the north shore of Lake Ontario,

and the Belly River in the North-west Territory. Beneath the Lower Boulder-clay the Palæozoic rocks are glaciated; and striæ and boulders alike indicate movement from north-east to south-west, from the Atlantic up the valley of the St. Lawrence. The Boulder-clay, which by damming the more ancient valleys, forms the basins of the great Canadian lakes, is of similar character to the material of the Missouri coteau or prairie escarpment of the west, which is regarded as the deposit at the margin of a sea laden with floating ice. The Lower Leda-clay is like the deposits now forming beneath the ice in Baffin's Bay and the Spitzbergen Sea. But the Upper Leda-clay appears to indicate more temperate conditions, since nearly all its fossils have been dredged in the Gulf of St. Lawrence; and the land plants which it yields still live on the north shore of the St. Lawrence. Some of the Leda-clays occur 600 feet above sea-level, while the beds include littoral gravels and sand, so that considerable changes of level are associated with their deposition. Subsequently extensive local glaciers clothed the Appalachian chain in the east, and the Cordilleran region of the west, where the deposits have been especially investigated by Dr. G. M. Dawson, and are regarded as indicating as great an elevation of land during the Upper Boulder-clay as was attained during the Lower Boulder-clay. Only when the Cordilleran region was at its maximum elevation the region of the great plains experienced a correlative subsidence and submergence; while the intervening subsidence between the two glaciations of the Cordilleran region is associated with the elevation of the plains, which led to the formation of great lakes in which inter-Glacial deposits and peat were formed. The second elevation of the west was followed by a partial subsidence to a level of about 2500 feet, with a long stage of stability during which white muds were deposited, and when the renewed elevation took place the shore-line stood about 200 feet lower than at present.

The Lower Boulder-clay over a great part of Canada is thickly packed with boulders; though in Triassic and Upper Carboniferous areas it becomes an incoherent sand. The stones are often scratched and grooved. They are mostly from the neighbouring rock-formations. In the lower valley of the St. Lawrence they are chiefly Laurentian gneiss from the north-east. The clay fills up valleys and depressions, and is thin or absent on the high ground. The striations beneath the Boulder-clay in New England run to the south-east, but there is also a south-west direction, a south direction, and occasionally the striæ are east and west. The north-east to south-west direction is attributed to the Arctic current, especially when associated with marine organisms. But where the striæ are upon mountains they are attributed to land-ice. Thus, on the south of the St. Lawrence, the Notre Dame Mountains show striæ which descend south towards the Baie des Chaleurs, and north towards the St. Lawrence. The phenomena indicate oscillations in level of the land.

The excavation of the basins of the great American lakes in the softer members of Silurian and Devonian formations is said to be

obviously due to atmospheric and river erosion during the Pliocene period, supplemented by the flow of cold ocean currents over the American land during submergence; so that the lake basins are of the same nature as the deep hollows which prolong the mouths of American rivers beneath the ocean along the American coast.

The third and fourth chapters discuss the physical and climatal conditions of the Glacial period in Canada.

Drawing a map of North America in the Pleistocene period, Sir William Dawson commemorates some of his contemporaries by giving their names to physical features of the period. "The great southern bay at the bottom of which lies the 'terminal moraine' may bear the name of Dana; the strait leading to the north-east, where the St. Lawrence now flows, may be Upham Strait; the great western opening may well be called Chamberlain Sound; and the northern bay filled with ice, in the region now occupied by Hudson's Bay, may be the Gulf of Wright. The great islands will be respectively Cordilleran and Laurentide lands, fit companions to Greenland; and the smaller eastern island, Appalachia Infelix."

But although the author uses the expression "terminal moraine," he urges that the existence of an ice-sheet which could have formed it is a physical impossibility, because there could not be sufficient evaporation and precipitation to afford the necessary snow in the interior; and secondly, because there is evidence of a depression which admitted Arctic currents through gaps in the Laurentian watershed, and down the great plains between the Laurentian area and the Rocky Mountains, as well as into the valley of the St. Lawrence. This does not in any way affect the author's belief in the great local glaciers described by Dr. G. M. Dawson, which occupied the Cordillera of British Columbia and discharged into the Yukon Valley, into Puget Sound, and into the Pacific. The former existence of glaciers on the Laurentian axis is accepted on the evidence from the Notre Dame region and the central districts of Newfoundland. The great V-shaped Laurentian axis is stated, on the evidence of the glacial striæ, to have thrown off ice on the south-east to the St. Lawrence Valley, and to the south-west towards the great plains, and to have poured its ice into the interior of Hudson Bay and the Arctic Sea. There is some evidence of a terminal moraine along the middle of Hudson Bay, which may have belonged to the inter-Glacial period.

The striation of the rocks at lower levels is attributed to the grating of pebbles included in shore-ice upon the rocky floor beneath, as the ice moved with the tide; this conclusion being based mainly on the observations of the late Dr. John Rae and of Captain Feilden, in the Arctic regions. The longitudinal direction of the striæ is attributed to the drifting of the ice backward and forward with tidal currents. It is stated that the striation produced by the sea is always accompanied with much smoothing and polishing, that the striation is not quite uniform in direction and often presents two sets of striæ, while land glaciers usually produce deep grooves as well as striæ which are more uniform in direction. It is fully

admitted that it may be difficult to distinguish the effects of grounding icebergs from those of land-ice. Charles Darwin's theory of the transport of boulders, from lower to higher levels, by floating ice, on a subsiding land, affords the only satisfactory explanation, it is urged, of the occurrence of erratics in Eastern Canada at a higher level than the rocks from which they were derived. Dr. G. M. Dawson is quoted as stating that he finds himself in agreement with Dr. Hector and Dr. Hayden in attributing the glacial phenomena of the great western plain of Canada to the action of floating ice, though the rounding, striation, and polishing of the Laurentian plateau, in the region of the Lake of the Woods, is to be attributed to the ice of glaciers. Such a glacier, covering the Laurentian uplands, liberated icebergs, which, depositing boulders and sediment, formed the highlands of the second plateau, including the Touchwood Hills, Moose Mountain and Turtle Mountain. The changed climatal conditions are regarded as mainly attributable to changes in the level of land; and it is believed that the present climate of Canada is separated from that of the Glacial age by one somewhat warmer, which coincided with an elevated condition of land. The date of the Glacial period was anterior to the excavation of the Niagara gorge, which is estimated to have required from 12,000 to 15,000 years.

The fifth chapter gives a summary of local details in the regions of Canada in which glacial phenomena have been observed; and the sixth chapter is an account of the fossils found in the deposits, illustrated with figures. There is a final summary of conclusions.

The book is illustrated with useful sections and woodcut maps. Its teaching is valuable from the breadth of country examined, the variety of phenomena considered, and the breadth of view which seeks to explain each set of facts by causes which are in harmony with the physical conditions evidenced in the several regions of Canada; and although the tone of the book has more that of an advocate recapitulating the steps of his exposition, now extending over a long period of time, than may be necessary in an impartial history of the Ice-Age, the story is well told, interesting in every page, and forms a valuable epitome of laborious work.

II.—Second Expedition to Mount St. Elias, in 1891. By Israel C. Russell, United States Geological Survey. Extract from the Thirteenth Annual Report, 1891–92. Pp. 91, Maps and Plates iii.–xxi. and 6 Figures. (Washington, 1894.)

THIS Report contains the observations of a small exploring expedition, under the charge of Mr. Russell, which revisited in 1891 the glaciated regions between Mount St. Elias and the sea-coast west of Yakutat Bay, in Southern Alaska. One of the objects of the expedition was to attempt to reach the summit of this mountain, but the difficulties proved too great and no higher elevation than 14,500 feet was reached. Many valuable and important observations relating to the structure of the glacier, its drainage, the nature of the moraines, and other points were

obtained, and a highly graphic description of these features is given in the Report, which deserves the consideration of all interested in glacial geology.

Mount St. Elias, from the data obtained by Mr. Russell, has an elevation of about 18,100 feet. It is the highest peak of a mountain range extending westwards from Yakutat Bay, which forms the gathering ground of an immense glacier, known as the Malaspina glacier. The front margin of this glacier is continuous for a distance of 70 miles; it covers the greater part of the area of comparatively low ground, from 20 to 25 miles in width, which extends from the base of the mountains southwards to the coast of the Pacific. The entire area of the glacier is estimated at 1500 square miles. One main lobe of the glacier extends into the open ocean and forms magnificent cliffs of ice, which are undermined by the waves, so that large masses break off and give rise to numerous icebergs. Other portions terminate before reaching the coast and from their margins numerous streams issue, highly charged with mud and gravel. The main body of the glacier forms a vast plateau of ice, the surface of which, at about five or six miles from its outer margin, has an elevation of approximately 1500 feet above the sea; the central portion is of clear white ice, free from moraine and generally undulating. It is traversed by countless numbers of crevasses. This clear central area is bounded on the south, that is, in the direction of the flow of the glacier, by a broad dark band of boulders and stones, known as the "barren moraine," and beyond this belt is a forest-covered area, in some places four or five miles in width. This forest region appears to have been covered by trees for a long period of time; they principally consist of spruces, some reaching three feet in diameter; cotton-wood trees; alders from 20 to 30 feet in height, besides a considerable variety of shrubs and bushes, and an undergrowth of rank ferns; the whole forming a mass of vegetation so dense as to be nearly impenetrable, and in order to pass through it a trail had to be made with axes and hatchets. But the most remarkable fact in connection with this forest-belt is that the morainic material, on which the trees grow, actually rests upon the glacial ice, and in some places this ice beneath the forest-covered moraine is estimated to be not less than 1000 feet in thickness. The area of the Malaspina glacier covered by forest is probably between 20 and 25 square miles. The moraine supporting the forest growth consists of boulders, earth and stones, which cover the glacier to a depth of not more than 3 or 4 feet as a rule. It only differs from the "barren moraine" in having a greater proportion of finer material between the larger stones and in the presence of a certain amount of humus derived from the vegetable growth. These forests only occur on those portions of the glacier where the ice appears to be stagnant and without motion. The occurrence of this forest-covered moraine resting on the surface of the glacier tends to throw considerable light on the origin of the ice-cliffs or "ground-ice formation" along the northern coasts of Alaska, in which the solid ice is overlaid by a layer of clay con-

taining Mammoth remains, and this is capped by a peaty layer supporting the vegetation of the region.

Another peculiar feature of the stagnant margin of the Malaspina glacier is the presence of numerous lakelets from 30 to 60 yards in width and bounded by steep walls of ice from 50 to 100 feet in height. These lakelets are supposed to have originated by the melting back of the walls of crevasses.

Mr. Russell further describes the geology of the Chaix Hills, a range from 8 to 10 miles in length and about 3000 feet in height, which project from the sea of ice between the coast and Mount St. Elias. The strata have a gentle dip to the north, and appear to be between 4000 and 5000 feet in thickness. They consist throughout of a sandy clay, in which are numerous stones and boulders, up to 8 feet in diameter; some of these latter are polished and striated. In the finer beds molluscan shells occur, belonging to several species which still exist in the adjoining ocean. These beds are evidently formed of morainic materials deposited about the extremity of a glacier which terminated in the ocean, and similar beds are probably now being deposited in the ocean in front of the western lobe of the Malaspina glacier. The Samovar hills, more to the north-east than the Chaix range, likewise consist of stratified morainic material. Many of the boulders in these deposits are of various kinds of crystalline rocks, which must have been brought from the north by glaciers. This great thickness of stratified morainic material, originally laid down beneath the sea, and now in part raised to an elevation of over 3000 feet, will give an idea of the long period of time which has elapsed, and of the great physical changes which have taken place since glaciers first left traces of their movements in these regions.

The maps and reproductions of photographs which accompany the Report afford realistic pictures of the salient features of the region described. G. J. H.

III.—Eleventh Annual Report for the Year 1891 of the State Geologist, Transmitted to the Legislature of New York, January, 1892. By James Hall, State Geologist. With an Illustrated Handbook of the Brachiopoda. 8vo. pp. 300, 22 Plates, 286 Woodcuts. James B. Lyon, State Printer. (Albany, 1892.)

AN *early* copy of the Report reached us last month — a delay in publication alike unjust to the authors, and a loss to the scientific public, as this work is one of great general utility, comprising the first part of a new illustrated handbook of the Brachiopoda. Besides the formal Report and Catalogue of additions to the collections of the State Museum of New York, the volume contains a list of the types and figured specimens of the Crustacea in the palæontological collections, and a paper by Prof. John M. Clarke on *Cordania*—a proposed new genus of the Trilobita—which can be no longer referred to Barrande's misnamed genus *Phæton*, to his subsequently substituted *Phætonides*, nor to Corda's *Prinopeltsi*.

The type of *Cordania* is *Phætonides cyclurus*, Hall, a form "allied to *Proetus* in the structure of the pygidium and thorax, to *Cyphaspis* in the cephalon generally, and to *Arethusina* in its glabella in particular." Its occurrence is first noted in the Lower Helderberg of New York, with an upward range through the Hamilton group of strata.

The bulk of this Report, viz. nearly two-thirds of the text of 300 pages, all the 22 plates, and 286 intercalated woodcuts, many of which are original productions, is devoted to "An Introduction to the Study of the Brachiopoda," by James Hall, assisted by John M. Clarke, and dedicated to the use of American students. It forms not only an epitome of vol. viii. part i. of the Palæontology of New York, with a general elementary introduction preceding the clear and concise generic definitions well suited to the comprehension of students in general; but, in fact, the new handbook combines, so far as it goes, all the best features of the previous more technical and restricted summaries of Davidson, Zittel, and Œhlert, *plus* the great knowledge, experience, and literary facility of Hall and Clarke. Although the "Report" was presented to the Senate of New York January, 1891, it is well up to date, and references to various important observations recorded in 1893 are not lacking. The concluding part, dealing with the Spire-bearers, Rhynchonelloids and Terebratuloids, it is stated, will be ready for Press in October, and will not, we trust, meet with any untoward delay in publication. Part I. treats of all the Inarticulata, *Paterina* included, and of the Articulata from the Orthoids to the Productoids, and affords ample proof of its value to students as an authoritative statement of elementary facts concisely presented and elaborated and explained by excellent illustrations of structural characters.

It presents all the essential facts of the general history, distribution, shell-structure, and anatomy, with such details of embryological phases as are needed to emphasize the importance of ontogeny in clearing up obscurities in the geological and genealogical history of the class in general and of genera in particular—such a manual, in fact, as could be produced only by an expert palæontologist of great experience like James Hall. We trust further handbooks on other invertebrate groups may be forthcoming from the same source.

The first part contains a map of the distribution of the recent species and a list, alphabetically arranged, of the localized provincial faunas based on Œhlert's work of 1876–1880, amplified by later discoveries. We would suggest the addition of a chart of specific bathymetrical limit (the data could be easily derived from the publications of Davidson, Œhlert, Monterosata, and others); this, with a table of geological range of genera, would localize all needful information in one handbook. Verily the students of the Brachiopoda in the twentieth century will have great reason to be grateful to the specialists of the nineteenth who have thus consolidated the results of general researches.

Systematic classification should be the natural outcome and termination of research, not hard and fast limits outlined at the beginning, within which facts discovered later must be enclosed. As Prof. Huxley

has tersely stated, the power of repeating a classification does not always imply any real knowledge of a group in a student. They are useful summaries to work by, and we expect from Prof. Hall and his indefatigable assistant, Prof. J. M. Clarke, some definite statement on this vexed point in conclusion. At present they stand alone in the rejection of family groupings of genera in name, although they seem tacitly to admit the necessity of some such grouping by writing persistently of "orthoids," "productoids," "terebratuloids," etc. This is all very well in its way, but it imposes on the student the burden of defining whether "orthoid," and other generic and subgeneric mutations, which have been described collectively by other authorities as the "Orthidæ," "Productidæ," etc., is intended to be understood, or whether the term "orthoids" is meant to define merely the eight species to which that genus is by Hall restricted.

These remarks are not offered in any carping critical spirit. It would be absurd to deny the right of the State Palæontologist of New York to formulate a new classification; no one can have a better claim in Europe or America. We note the point as a difficulty in generalization and likely to puzzle the students to whom this handbook of the Brachiopoda is addressed, and one, therefore, that should be removed in the concluding portion of this most praiseworthy manual. We have noticed the work at some length, because one would scarcely expect to find a handbook for students enclosed within the covers of an annual Museum Report. Science just now must be in "a parlous state," indeed, in America, when scientific men of acknowledged repute and standing are driven to adopt such a roundabout method of publication. It is to be presumed that Hall's Handbook to the Brachiopoda will be published separately on its completion. AGNES CRANE.

IV.—THE PAST WORLD AND ITS EVOLUTION. (Die Vorwelt und ihre Entwicklungsgeschichte.) By Dr. ERNST KOKEN. Royal 8vo. pp. 654, 117 Figures in the text, and two Maps (Plates I. and II.). (Leipsic: Weigel, 1892.)

IN the preface the author wishes us to understand that he addresses himself not to specialists, but to a wider circle of readers; we venture, however, to predict that, like Neumayr's Erdgeschichte, the present book will, before long, be found as well in the libraries of geologists and palæontologists as in those of the *Gebildete Kreise* generally.

The three first chapters (The Interior of the Earth and the Hardened Crust; The Formation of Mountains; The Notion of Time in Geology) are a sort of introduction, and are almost exclusively geological in their contents, as is chapter xii. (Quaternary and Ice Period). Chapters iv.–xi. are dedicated to the different "systems," beginning with the Cambrian and ending with the Tertiary; in these the palæontological part predominates. Chapter xiii. is dedicated to the Animal World in Quaternary times.

The author is not only conversant with the very latest work done in *every* department of the wide field covered by his subject—in this

respect a comparison with Neumayr's book before mentioned shows how much nowadays even the lapse of seven years may affect the progress of science—but, what strikes us even more, so much elaborate thought and such original views are displayed in every chapter, that we admire the author as much when we dissent from, as when we agree with, him.

In the concluding chapter we are expressly told, what the attentive reader must already have become aware of, namely, that the fundamental view which guided the author is a decided uniformitarian one. The remainder and larger portion of the same chapter is dedicated to a discussion of the Evolution theory; here Dr. Koken strongly opposes the overwhelming part ascribed to natural selection.

The two maps are very acceptable, being reconstructions of the continents and seas during the Cretaceous (plate i.), and during the older Tertiary, together with an illustration of the extension of the Pleistocene Ice period in the northern hemisphere (plate ii.). They are in the main on the plan of Neumayr's reconstruction of Jurassic continents, which is also added by means of black dotted lines on the first map. Dr. Koken's maps serve not only as illustrations to the respective chapters of the text, but they are, as it were, graphic summaries of the same; they will be particularly appreciated by those who have hitherto sought in vain for some guidance of this kind when working on geographical distribution of organisms. The figures in the text are first-rate.

An English translation would prove a success to an enterprising publisher, and it might not be indispensable to many scientific workers, for the book ranks with Neumayr's *Erdgeschichte* and Prof. Suess' *Antlitz der Erde* in its clear as well as fascinating writing.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—April 25th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair.

Mr. A. R. Sawyer, referring to specimens exhibited by him from the Transvaal, Orange Free State, Cape Colony, Mashonaland, and Matabeleland (the last-mentioned collected during the recent war), remarked that gneisses and gneissose granites cover a large portion of Mashonaland, together with patches of schistose rocks and a few stratified rocks. He drew attention to the fantastic shapes assumed on weathering by the granitic gneiss, which he considered solely due to atmospheric agencies, and not to ice-action or to the effects of submersion.

The schistose rocks are, for the most part, sheared and altered igneous masses. There are numerous examples of dolerites and epidiorites passing into hornblende-schists, and of more acid igneous rocks. Masses of magnetite occur in various parts of Mashonaland, and serpentinous rocks (which probably owe their origin to the alteration of peridotites) in the north-west corner of the Victoria Gold-field.

Extremely auriferous veins occur amongst the sheared acid igneous rocks of the Umhungwe Valley in the Manica district, and gold occurs in the kaolin produced by the disintegration of these rocks. The following communications were read :—

1. "Further Notes on some Sections on the New Railway from Romford to Upminster, and on the Relations of the Thames Valley Beds to the Boulder Clay." By T. V. Holmes, Esq., F.G.S.

The author alludes to his discovery of Boulder-clay on this new railway at Hornchurch (dealt with in a previous paper, Q.J.G.S. August, 1892), and describes the finding of more Boulder-clay close to Romford during the deepening and widening of a cutting there. The Boulder-clay was on precisely the same level as that at Hornchurch, a mile and a half to the south-east, and, like it, was covered by gravel belonging to the highest, and presumably oldest, terrace of the Thames Valley system. A portion of the silted-up channel of an ancient stream-course was also found in this Romford cutting. Its relations to the Boulder-clay could not be seen, as they were not in contact, but they were alike covered by the oldest gravel belonging to the Thames Valley system. The author discusses the probable direction of the flow of this stream-course, and the way in which it was superseded by the ancient Thames. After noticing certain points brought forward during the discussion on his former paper, he concludes with a criticism on the views to which Dr. Hicks inclines in his paper on the Sections in and near Endsleigh Street (Quart. Journ. Geol. Soc. vol. xlviii. 1892) as regards the age of those beds, asserting that they are, in all probability, simply River Drift of the Thames Valley system, and consequently post-Glacial, in the sense of being later in date than the Boulder-clay of Essex and Middlesex.

2. "On the Geology of the Pleistocene Deposits in the Valley of the Thames at Twickenham, with Contributions to the Flora and Fauna of the Period." By J. R. Leeson, M.D., F.L.S., F.G.S., and G. B. Laffan, Esq., B.Sc., F.G.S.

The section described in this paper was exposed during the construction of an effluent from the Twickenham sewage-works to the Thames. Its length was about one mile.

The beds exposed were :—(1) Coarse reddish-yellow gravels, coloured blue below, lying on an eroded surface of (2) Dark-blue loam, varying in thickness, the greatest thickness seen being 3 feet, at a place where the bottom was not reached; (3) Dark sand; (4) Coarse ballast-gravel; (5) London Clay.

The loam (which is quite a local deposit) yielded eight species of Mollusca and fourteen species of plants, all still living in the neighbourhood. A number of Mammalian bones referable to seven species were lying just on the surface of the loam. Amongst the forms were Bison and Reindeer.

The authors consider that the loam was deposited in a small lake, and they allude to similarities between it and a deposit described by Dr. Hicks as occurring in the Endsleigh Street excavations.

3. "On a Goniatite from the Lower Coal-measures." By Herbert

Bolton, Esq., F.R.S.E. (Communicated by George C. Crick, Esq., F.G.S.)

Sowerby in his "Mineral Conchology" figures two fossils under the name of *Goniatites Listeri*, of which the left-hand figure is clearly *G. Listeri*, whilst the right-hand one differs considerably from it. The author gives diagnoses of *Goniatites Listeri* and of a supposed new species, which agrees with the form represented in Sowerby's right-hand figure. This species, noticed by the author, is limited to the shales forming the roof of the "Bullion" or Upper Foot seam of the Lower Coal-measures, whilst *G. Listeri* ranges from the Lower Limestone Shales to the "Bullion" seam.

II.—May 9th, 1894.—Dr. Henry Woodward, F.R.S., President, in the Chair.

Mr. Lydekker exhibited some specimens from a collection of Argentine fossil Vertebrates which he had been allowed to select from the La Plata Museum for presentation by Dr. Moreno to the British Museum. Many of these belonged to types previously quite unrepresented in the collection of the latter. He also drew attention to his recently published monograph on Argentine Vertebrates, and stated that he hoped these results of his journey to La Plata, under the auspices of the Royal Society, would be regarded as satisfactory. The difficulty he himself had laboured under in endeavouring to understand what had been previously written in regard to the extinct mammals of Argentina was largely due to the absence of satisfactory figures; and his object had, therefore, been to figure as many specimens as possible on a large scale, in order that others might have an opportunity of judging for themselves, quite apart from his own descriptions and conclusions.

He had undertaken his previous journey somewhat unwillingly, at the wish of Sir W. H. Flower and Mr. Sclater; but he had been so interested in what he had seen that he hoped means might be afforded him of repeating his visit this year.

The following communications were read:—

1. "Carrock Fell: a Study in the Variation of Igneous Rock-masses.—Part I. The Gabbro." By Alfred Harker, Esq., M.A., F.G.S.

The paper opens with an account of the general relations of the intrusive rock-masses of the district, and proceeds to deal more particularly with the gabbro, which forms the earliest intrusion.

A petrological description of the Carrock Fell gabbro is followed by a study of the variations observed in different parts of the mass. The rock becomes progressively more basic from the centre to the margin, passing from a quartz-gabbro with as much as $59\frac{1}{2}$ per cent. of silica to an ultrabasic type with as little as $32\frac{1}{2}$. The latter in extreme cases contains nearly 25 per cent. of iron-ores, partly titaniferous. This is compared with the igneous iron-ores described by Vogt in Scandinavia, etc., and the probable physical cause of the remarkable variation in the gabbro is discussed.

Other modifications of the gabbro are briefly noticed, due on the one hand to metamorphism of the rock by a somewhat later

intrusion of granophyre, on the other hand to the gabbro-magma having enclosed considerable masses of the basic lavas of the district, which are themselves highly metamorphosed.

2. "The Geology of Monte Chaberton." By. A. M. Davies, Esq., B.Sc., F.G.S., and J. W. Gregory, D.Sc., F.G.S.

The importance of the Chaberton district, as affording a key to the general geology of the Cottians, is explained, and the opinions of previous observers referred to. The mountain was examined from three sides—that of the Grand Vallon; the approach from Mont Genèvre by the Col de Chaberton; and that of the Clos des Morts Valley. The following are the conclusions arrived at:—

(1) The well-known Chaberton serpentine is intrusive into the calc-schists, and yields fragments to the *carqueules* of the Trias; it is, therefore, a *pre-Triassic* intrusion.

(2) There are on the mountain other fairly basic schistose rocks (quartz-chlorite-schists) which cut the Trias, and are therefore *post-Triassic.*

(3) The contorted beds in the Clos des Morts Valley are fossiliferous limestone, and it is from them that the fallen blocks previously recorded were derived. The only recognisable fossil is *Calamophyllia fenestrata,* Reuss, a characteristic coral of the Gosau beds. In spite, therefore, of the doubts of Kilian and Diener, the opinion expressed by Neumayr as to the existence of Cretaceous rock in this part of the Alps is confirmed.

(4) The earth-movements of the mountain are described; they include ordinary folds, inversions, faults, and an important thrust-plane.

(5) It is suggested that in addition to the two series of intrusive rocks above-mentioned as pre- and post-Triassic, a third series of late Cretaceous or Tertiary date may be represented in the Mont Genèvre and Rocciavré masses.

3. "Cone in Cone. How it occurs in the Devonian (?) Series in Pennsylvania, U.S.A., with further details of its Structure, Varieties, etc." By W. S. Gresley, Esq., F.G.S.

The author describes cone-in-cone structure occurring in the Portage Shales of Pennsylvania, and gives details concerning the nature of the structure as seen in these shales. He criticizes the explanation of Mr. J. Young as to the origin of the structure, and concurs in a great measure with the views of those who have suggested that the formation was due to pressure acting on concretions.

CORRESPONDENCE.

A PHYSICAL CONTRIBUTION TO DYNAMIC METAMORPHISM.

Sir,—Among recent physical researches, one of the most interesting to geologists is embodied in two papers by Prof. M. Carey Lea on "Endothermic Reactions effected by Mechanical Force."[1] The author shows by direct experiment "that mechanical force can bring about reactions which require expenditure of energy, which energy

[1] Amer. Journ. Sci. 1893 (3), vol. xlvi. pp. 241–244, 413–420.

is supplied by mechanical force precisely in the same way that light, heat, and electricity supply energy in the endothermic changes which they bring about."

The experiments consisted chiefly in the reduction of silver, mercury, platinum, and gold, from their salts. The result was indicated in each case by a darkening of the powder, and in one case, sodium chloraurate, the reduced gold was separated and weighed.

In a first series of experiments the substance operated upon, wrapped in platinum or silver foil, was subjected to a pressure of about 70,000 atmospheres by means of a specially devised apparatus. More interesting, however, is a second series of experiments, in which similar results were obtained by grinding the powder by hand in a stout porcelain mortar, so as to give a shearing motion. The important conclusions drawn are:

(i.) That in these experiments the mechanical energy does not undergo any intermediate conversion into sensible heat. The operation may be conducted slowly or intermittently, and the apparatus does not become warmed.

(ii.) That shearing stress is enormously more effective than simple pressure.

These two points are well illustrated by the behaviour of mercuric chloride. A simple pressure of 70,000 atmospheres was not sufficient to produce any change, but trituration in a mortar for fifteen minutes caused a very evident reduction to calomel. In this instance the decomposition is one which *cannot be produced by heat.*

Although the chemical transformations involved in dynamic metamorphism are of a more complex kind than those here noted, it seems fair to conclude that, in so far as they are endothermic, they may be brought about by mechanical force only, without the intervention of heat, and that the most marked effects of this kind are to be looked for where shearing stress has been brought into play.

St. John's College, Cambridge. Alfred Harker.
May 4th, 1894.

THE DEVONIAN VOLCANIC ROCKS OF START BAY.

Sir,—In my paper "On Certain Affinities between the Devonian Rocks of South Devon and the Metamorphic Schists" (Geological Magazine, June, 1892), no attempt was made to define the horizon of any of the Devonian rocks themselves. The schists were merely referred to unaltered rocks whose position had been elsewhere declared.

In his recent address to the Geological Society, Mr. W. H. Hudleston, in noticing my paper, remarks that Lower Devonian diabases "in some districts are not by any means in evidence" (Proc. Geol. Soc. vol. 50, p. 130).

As a matter of fact the exact horizon of the Devonian diabases in Start Bay (to which the metamorphic green rocks were referred) does not affect my notes concerning them, which merely go to show that the green rocks at the Start and neighbourhood are of about the same age as the said Start Bay diabases, whether the latter be Lower, Middle, or Upper Devonian.

The stratigraphical questions of Start Bay lie entirely outside my province; but I may, perhaps, be allowed to point out how little they affect my position.

In the course of a walk from near the Start Lighthouse to Dartmouth, we may notice the following rocks:—Westward of the lighthouse, forming the cliff face, is a mass of compact greenrock, which, previously to Mr. Ussher's survey, seems to have been invariably overlooked by geological visitors. Immediately to the north are the mica-quartz-schists of Start Ridge, succeeded along the coast by a series of mica-schists which pass into the ordinary Devonians at Hallsands, with but slight indications of an important area of greenrock inland.

Devonian slates and sandstones are interrupted by volcanic rocks at Torcross, and then continue to Blackpool, where volcanic rocks reappear in force, and are considerably developed up to the entrance of Dartmouth Harbour, where the Mewstone islet and the eastern and western blackstones indicate the seaward extension of the diabases.

Now, from Hallsands to Dartmouth Harbour, the rocks including the diabases are admitted to be Devonian, of some age. If they are Lower Devonian, Mr. Hudleston tells us that in other districts Lower Devonian diabases "are not by any means in evidence." If, on the other hand, they are Middle Devonian, it is equally noticeable that the limestones (the characteristic rocks of that horizon) are absent.

If the explanation be that Middle Devonian slates and diabases are folded together with Lower Devonian slates and sandstones (with no useful fossils to assist the observer) the difficulty of the case as a purely Devonian problem is sufficiently obvious. But so far as the main problem of the age of the metamorphic green rocks is concerned, the question of the exact horizon of the Start Bay diabases, whether Lower or Middle Devonian, is of very minor consequence or interest. My point is that the sandstones, slates, and diabases north of the metamorphic boundary are the analogues of the quartz-schists, mica-schists, and green rocks to the south of that boundary; of which fact I do not entertain the shadow of a doubt.

A. R. Hunt.

OBITUARY.

CHARLES S. BEACHLER.

Born November 5th, 1870. Died April 5th, 1894.

We greatly regret to hear of the death of this young American geologist, which occurred at Crawfordsville, Indiana, on the above date, and was due to a complication of heart and lung trouble. Charles Beachler's scientific studies were begun at Wabash College, when he was fifteen years old. Here he became interested in herpetology, and made a collection of the reptiles of Indiana for the College at his own expense. Subsequently he was employed by Mr. Frank Springer to collect Crinoids from the Carboniferous beds of Crawfordsville, and the Niagara Limestone of Waldron and

St. Paul. While engaged in this work he made the best of his opportunities to study the geology of the districts visited, and made some geological excursions on his own account.

Short but interesting notes resulting from these investigations have appeared in the American Naturalist, American Geologist, and the Journal of Geology. In the autumn of last year Mr. Beachler studied and taught under Prof. J. W. Spencer, State-Geologist of Georgia, and, on his return to Crawfordsville, himself became a candidate for the post of State-Geologist of Indiana.

MISCELLANEOUS.

"Harkness Scholarships in Geology for Men and Women."—It seems difficult to devise a more pleasing or useful form in which to perpetuate the memory of one who was greatly esteemed and loved, than to establish a scholarship, as has been done by the late Mrs. Pearson of Penrith, to the memory of her brother, Professor Robert Harkness, F.R.S., F.G.S., late Professor of Geology, Queen's College, Cork.

Professor Harkness, who died in 1878, was a graduate of the University of Edinburgh, where he attended the lectures of Professors Jamieson and James D. Forbes. He was appointed to the Chair of Geology in Queen's College, Cork, in 1853, a post which he held until 1878. He was an excellent geological teacher, and contributed many admirable papers on the geology of Cumberland and Westmoreland, and other parts of England and Ireland. He was, for many years, a constant attendant at the meetings of the British Association, where his presence in Section C was as regularly expected as that of the late Mr. William Pengelly, F.R.S., and he took an active part in its meetings and discussions, as well as in those of the Geological Society whenever he could be present.

The scholarship for women, tenable at either Girton or Newnham College, Cambridge, is to be awarded triennially to the best candidate in an examination in Geology and Palæontology, provided that sufficient proficiency be shown. The candidates must be resident members of either Girton or Newnham College, in their first or second term. The scholarship will be of the value of about £35 a year tenable for three years. The next examination will be held at Cambridge in the Michaelmas Term, and the award will be made on or before November 15th, 1894.

Another Harkness Scholarship in Geology, which was also established by Mrs. Pearson to her brother's memory, is open to men and is vested in the hands of the University of Cambridge. This is awarded annually, any member of the University being eligible who has graduated as B.A., "provided that not more than three years have elapsed since the 19th day of December next following his final examination for the degree of Bachelor of Arts." The award is in the hands of a Board of Electors. We trust that this memorial to so excellent a geologist may long maintain its vitality, and form a living link between the past and present for the good of our science.